**UNITED STATES ENVIRONMENTAL PROTECTION AGENCY**
**REGION II**
**290 BROADWAY**
**NEW YORK, NEW YORK 10007-1866**

May 9, 2017

BY ELECTRONIC MAIL

Robert Law, Ph.D.
CPG Project Coordinator
de maximis, inc.
186 Center Street, Suite 290
Clinton, New Jersey 08809

Re: Lower Passaic River Study Area (LPRSA) Draft Remedial Investigation Report – Administrative Settlement Agreement and Order on Consent for Remedial Investigation/Feasibility Study (Agreement) CERCLA Docket No. 02-2007-2009

Dear Dr. Law:

The U.S. Environmental Protection Agency (EPA) has reviewed the Cooperating Parties Group's (CPG) February 9, 2017 responses to EPA's April 14, 2016 comments on the CPG's February 2015 Draft Remedial Investigation (RI) Report. In accordance with Section X, Paragraph 44(d) of the Agreement, EPA has enclosed an evaluation of CPG's comments responses with this letter.

Please proceed with revisions to the draft RI Report consistent with the enclosed comment evaluations and the January 2017 RI/FS Schedule. If there are any questions or clarifications needed on EPA's enclosed comment evaluations, please contact me to discuss.

Sincerely,

Jennifer L Poma

Jennifer LaPoma, Remedial Project Manager
Lower Passaic River Study Area RI/FS

Enclosure

Cc: Zizila, F. (EPA)
Sivak, M. (EPA)
Hyatt, B. (CPG)
Otto, W. (CPG)

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| 361 | Section 7 | General | | The CPG models presented in the draft RI include model code and input changes that represent significant modifications to the modeling framework. As stated in the 2007 Settlement Agreement and Order of Consent (EPA 2007) these changes require EPA approval. Unless and until EPA approves the modifications, the RI text should not state that the models conform to either the 2007 Settlement Agreement and Order of Consent or Modeling Work Plan. The CPG must revise their current approach to address the issues listed in **Attachment 3,** provide EPA the revised code inputs and outputs for their review and revise the RI text to indicate that modifications to the modeling framework are undergoing review by EPA. | The text will be revised to reflect the status of the model with respect to the Region 2 approval process.<br><br>The required model changes listed in Attachment 3 have been discussed at length with Region 2 during the HD/ST/OC/CFT modeling meetings on June 28, September 20, and December 16, 2016; in the bioaccumulation modeling meetings on June 29 and August 24, 2016; and in follow-up written correspondence and calls on specific topics. As a result of the meetings, the CPG will implement agreed upon changes to the models. The CPG has proposed revised approaches to address the majority of Region 2's concerns listed in Attachment 3, and CPG is working collaboratively with Region 2's modeling team to resolve remaining concerns. Further diagnostics and supporting information will be provided to Region 2 to facilitate approval. The revised code, inputs, and outputs will be delivered to Region 2 and the report text will be revised accordingly. | The response is accepted, pending review of the revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: RI Document<br>Sub-Categories: EPA Approval, Administrative Order, Workplan |
| 362 | Section 7 | General | | In general, the organization of the information presented could be improved. It is unclear why certain details are presented in Appendix K rather than in Section 7 or the model-specific appendices. Appendix K should be eliminated, and the information presented in Appendix K should be presented either in Section 7 of the RI Report or in the appendices specific to the individual models. | The requested structural change will be made. Appendix K will be eliminated, and the information in Appendix K will be presented in Section 7 of the RI Report or in the individual modeling appendices. | Response accepted, pending review of the revised RI and supporting appendices.<br><br>Categories: RI Document<br>Sub-Categories: Document Structure |
| 363 | Section 7 | General | | In general, the information presented in the figures should be described with greater detail either in the figure legends or in the text that references the figures. For example, Figure 3 in Appendix K has kilometers at the top of each panel and miles at the bottom; water depth in feet and meters on the left and right sides, respectively, of each panel; a color scale representing total suspended solids (TSS); and red lines representing isohaline contours. This is a large amount of information presented in a condensed format, and should be presented with a greater level of documentation. Please revise the report and figures to ensure that an appropriate level of documentation is provided to allow the reader to interpret the information presented in each figure. | Additional information will be added to clarify figures as deemed necessary and to incorporate feedback received during the ongoing interactions with the Region 2 modeling team. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 364 | Section 7 | General | | Because model results for different surface sediment depths (i.e., both 0-2 cm and 0-15 cm are used as the surface sediment depth), each text discussion and figure presenting surface sediment results should identify the depth interval over which the results were averaged. Please revise the report and figures to ensure that the surface sediment depth is clearly defined wherever surface sediment results are presented. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |

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| 365 | Section 7 | Specific | Section 7.1, page 1, first paragraph, fourth sentence, and Appendix K, Section 1, page 1, first paragraph, fourth sentence, Section 3.1.2, page 9, first paragraph, first sentence | As presented, the models, including the changes that have been incorporated into the contaminant fate and transport (CFT) model, have not been approved by EPA, and therefore do not comply with the 2007 Settlement Agreement and Order on Consent. Furthermore, there are deviations from the 2006 Modeling Work Plan (HydroQual 2006a, 2006b) in the models, including the carbon simplification, the lack of a contaminant hindcast, and the number of contaminants modeled. All deviations from the 2007 Settlement Agreement and the 2006 work plan should be described and justified in the text. | Changes in the CFT model, such as the fluff layer and the revised partitioning approach, and other model topics raised in these comments have been discussed with Region 2 during the modeling meetings cited in the Response to Comment 361. Discussions with Region 2 will continue so as to finalize the modeling approach on these topics. The final agreed upon changes to the model frameworks and inputs will be described in the revised report, with justification for the deviations from the 2006 Modeling Work Plan.<br><br>The CPG notes that Region 2 made a significant deviation from the 2006 Modeling Work Plan in the 8-mile ROD by not applying a bioaccumulation model, as called for by Section 6.5 of the Work Plan. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: RI Document<br>Sub-Categories: EPA Approval, Administrative Order, Workplan |
| 366 | Section 7 | Specific | Section 7.1, page 1, second paragraph, first sentence, and Appendix K, Section 1, page 1, first bullet | This sentence should be revised to include the Kill van Kull and Arthur Kill since these are included in the domain of the models. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 367 | Section 7 | Specific | Section 7.1, page 2, first paragraph | Please revise the text to state that the regional model was run by EPA and that all of the boundary inputs described were provided by EPA. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 368 | Section 7 | Specific | Section 7.1, page 2, second paragraph, sixth sentence | Please revise the text to note that navigation scour was included empirically in the model based on interpretation of bathymetric data in limited areas along the western side of the Passaic River below RM 1.5. Please add a figure presenting the cells where navigation scour was incorporated or reference Appendix M, Figure 34. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications, Navigation Scour |
| 369 | Section 7 | Specific | Section 7.1, page 2, second paragraph, last sentence, and Appendix K, Section 3.2, page 10, second paragraph, second sentence | Considerable effort was required to calibrate the sediment transport (ST) model, including the incorporation of new mechanisms in the model (bed forms, the fluff layer, and navigation scour); balancing bathymetry change data, water column solid data, and SEDflume data that suggested conflicting parameterizations for model inputs; adjustments to parent bed and deposited bed critical shear stresses, erosion rates, and layering; and modifications to sediment size classes represented in the model and their settling rates. The statement that "these inputs and parameters have been defined to a large extent by data from the LPRSA, with minimal adjustment during calibration" oversimplifies the very extensive effort that was required to calibrate the ST model to the complex and sometimes contradictory site data. Please revise the text to provide a more accurate description of the ST model calibration process. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |

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| 370 | Section 7 | Specific | Section 7.1, page 3, second paragraph, eighth and ninth sentences | Dissolved organic carbon (DOC) is not modeled. It is specified as a constant value and that value is not used in the organic carbon (OC), CFT, or bioaccumulation models. Additional demonstrations of the OC model's ability to reproduce the Sediment Transport – System Wide Eutrophication Model (ST-SWEM) and data have been requested by EPA, particularly longer-term runs and additional detail on spatial and temporal results, which have not been provided. Please refer to **Comment Nos. 517 to 534** for further information on the requested outputs and concerns with the carbon simplification approach. Please provide the additional analyses requested and modify the text to clarify that water column DOC was not modeled or used by the CPG. | The requested change will be made. | Response accepted pending review of the requested analyses, text, and figure revisions.<br><br>Categories: RI Document, OC Model, CFT Model<br>Sub-Categories: Text Clarifications, DOC, Partitioning |
| 371 | Section 7 | Specific | Section 7.1, page 4, second paragraph, fifth sentence and footnote 2 | The CPG approach of adjusting partitioning settings to prevent sorption to algal and DOC does tend to result in an increase in the contaminant mass returning to the sediment with the resuspended particles, which is the desired result. However, the way in which the inputs are specified in the model results in instantaneous desorption of contaminants from the sediment particles to the dissolved phase, with no algal uptake or sorption of dissolved-phase contaminants to DOC. The latter two processes would be expected to occur on time scales shorter than the desorption time scale. The partitioning approach used is not valid and needs to be corrected. Refer to **Comment Nos. 557, 562, 563, and 564** for further discussion of the partitioning approach presented in the RI. | The CPG is currently working collaboratively with Region 2 on a revised partition approach that includes sorption to all carbon forms and takes account of the desorption kinetics in the water column that mediate net transport of contaminant from the bed to the water column. The topic was discussed at each of the three modeling meetings noted in Comment 361, in notes exchanged on June 27, September 19, September 29 and December 15, 2016, and in a conference call with Region 2's modeling team on January 6, 2017. The CPG is currently considering two general partitioning approaches in the RCATOX framework: 1) a version based on the DiToro (1985) model (termed "SHREQ" in past interactions); and 2) a scale-factor approach suggested by Region 2 (termed "a_det" in past interactions). Two implementations within the former are being explored: a) a single-state variable approach and b) a two-state variable approach. It has been agreed during interactions with Region 2 to keep most partitioning parameters the same as in the FFS/ROD model, with the exception of the Koc, which will be based on the DiToro (1985) relationship of Koc ~ Kow.<br><br>The multiple frameworks are being evaluated in response to Region 2's concern that a single-state variable implementation of the DiToro model may cause artificial stripping of contaminant mass from the water column. Region 2 suggested the a_det approach as an alternative not subject to this concern, and CPG formulated the two-state variable implementation of the DiToro model as a way to assess and potentially address Region 2's concern. The behavior of all three sorption models is being assessed and will be discussed in a follow-up call with Region 2. It has been noted that the behaviors may be fairly similar, in which case | The response is accepted pending review and approval of the model revisions and revised text. Based on the CPG's presentation and discussions with EPA at the April 6, 2017 modeling meeting, it is EPA's understanding that the CPG is proceding with implementation of the two-state variable implementation of the DiToro model. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: CFT Model<br>Sub-Categories: Partitioning |

<table>
<tr><th>No.</th><th>Section</th><th>General or Specific</th><th>Page No.</th><th>EPA Comment – 4/14/16</th><th>CPG Response – 2/9/17</th><th>EPA Evaluation of Response – 5/9/17</th></tr>
<tr><td></td><td></td><td></td><td></td><td></td><td>the framework likely to be most defensible to a peer review would be selected (January 6, 2017 follow-up call).<br><br>Once the details of the approach are finalized through additional interactions, CPG will implement the new approach in the calibration and submit code, inputs, and results for Region 2's review.</td><td></td></tr>
<tr><td>372</td><td>Section 7</td><td>Specific</td><td>Section 7.1, page 4, second paragraph, sixth sentence, and Appendix K, Section 5.2, page 20</td><td>
<table>
<tr><th>Chemical</th><th>$K_{OW}$*</th><th>Chemical</th><th>$K_{OW}$</th></tr>
<tr><td>Tetra-PCB</td><td>6.00</td><td>1,2,3,7,8-PeCDD</td><td>7.37</td></tr>
<tr><td>3,3',4,4'-Tetra-PCB (BZ#77)</td><td>6.36</td><td>3,3',4,4',5,5'-Hexa-PCB (BZ#169)</td><td>7.42</td></tr>
<tr><td>3,4,4',5-Tetra-PCB (BZ#81)</td><td>6.36</td><td>2,3,3',4,4',5,5'-Hepta-PCB (BZ#189)</td><td>7.71</td></tr>
<tr><td>2,3,7,8-TCDF</td><td>6.54</td><td>1,2,3,7,8,9-HxCDF</td><td>7.95</td></tr>
<tr><td>2,3,7,8-TCDD</td><td>6.65</td><td>1,2,3,6,7,8-HxCDF</td><td>7.95</td></tr>
<tr><td>2,3,3',4,4'-Penta-PCB (BZ#105)</td><td>6.65</td><td>2,3,4,6,7,8-HxCDF</td><td>7.96</td></tr>
<tr><td>2,3,4,4',5-Penta-PCB (BZ#114)</td><td>6.65</td><td>1,2,3,4,7,8-HxCDF</td><td>7.96</td></tr>
<tr><td>2,3',4,4',5-Penta-PCB (BZ#118)</td><td>6.74</td><td>1,2,3,6,7,8-HxCDD</td><td>8.09</td></tr>
<tr><td>2',3,4,4',5-Penta-PCB (BZ#123)</td><td>6.74</td><td>1,2,3,7,8,9-HxCDD</td><td>8.10</td></tr>
<tr><td>3,3',4,4',5-Penta-PCB (BZ#126)</td><td>6.89</td><td>1,2,3,4,7,8-HxCDD</td><td>8.12</td></tr>
<tr><td>2,3,3',4,4',5-Hexa-PCB (BZ#156)</td><td>7.18</td><td>1,2,3,4,7,8,9-HpCDF</td><td>8.67</td></tr>
<tr><td>2,3,3',4,4',5'-Hexa-PCB (BZ#157)</td><td>7.18</td><td>1,2,3,4,6,7,8-HpCDF</td><td>8.67</td></tr>
<tr><td>2,3,4,7,8-PeCDF</td><td>7.23</td><td>1,2,3,4,6,7,8-HpCDD</td><td>8.82</td></tr>
<tr><td>1,2,3,7,8-PeCDF</td><td>7.25</td><td>OCDF</td><td>9.39</td></tr>
<tr><td>2,3',4,4',5,5'-Hexa-PCB (BZ#167)</td><td>7.27</td><td>OCDD</td><td>9.55</td></tr>
</table>
*Log $K_{OW}$ values from the FFS model (LBG 2014).<br><br>
Section 5.6 of the Modeling Work Plan (HydroQual 2006a) states: "The Lower Passaic River Restoration Project Contaminant Fate and Transport model calibration for HOCs will be based primarily on the ability of the model to reproduce measured concentrations (historical and current) of dioxin/furan congeners and coplanar PCB congeners in water and sediments." The calibration results presented are limited to one dioxin congener (2,3,7,8-TCDD) and total tetrachlorobiphenyl. While the two chemicals chosen for calibration represent a range of loading histories, with 2,3,7,8-TCDD contamination more closely related to the former Diamond Alkali facility and tetra-PCB more widespread, the range of chemical properties represented is narrow compared to the range of properties for the 29 dioxin, furan, and PCB congeners that are targeted for calibration based on the work plan text. As an example, the table below presents $K_{OW}$ values for these 29 chemicals plus tetra-PCB; as shown by the highlighted values, the two chemicals used for calibration of the CFT model both fall at the low end of the range of $K_{OW}$ values for the chemicals targeted in the work plan. At a minimum, the RI Report must present summary model calibration results for all 29 chemicals prescribed by the Modeling Work Plan.</td><td>In a memorandum transmitted to Region 2 on December 8, 2016, the CPG proposed to simulate a subset of COPCs in the CFT model. The COPC selection is based on an assessment of the 48 COPCs included in the Region 2 FFS/ROD model with regard to human health and ecological risks, the need to cover a wide range of hydrophobicity (Kow), and the availability of detected data.<br><br>The CPG has subsequently prepared additional regression analyses to support a reduced number of calibration COPCs, and they will be provided as a supplement to the CPG's December 8, 2016 memorandum. In particular, the surface sediment COPC regressions presented in the memorandum were repeated for subsurface sediment data to evaluate the potential for shifts in the relationships between COPCs over time, in response to Region 2's request at the December 16 modeling meeting.</td><td>The response is accepted pending review and approval of the proposed revision to the modeling approach. On April 12, 2017, EPA submitted a table to the CPG, based on an expansion of the table presented by the CPG in their December 8, 2016 memo. Once the CPG fills out that table identifying their proposed approach for addressing risk associated with all of the preliminary COPCs, EPA will be able to review the proposed approach and provie feedback on a path forward.<br><br>Categories: CFT Model<br>Sub-Categories: CoPCs</td></tr>
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| 373 | Section 7 | Specific | Section 7.1, page 5, first paragraph, third and fourth complete sentences and footnote 4 | Although it is likely that there is some small amount of OC associated with non-cohesive solids in the LPR, this was not included in the model because data are not available to directly quantify the amount of carbon that might coat a sand particle, and because of the expected limited fraction of contaminants transported with non-cohesive particles. LPR OC data are limited to bulk samples of mixtures of cohesive and non-cohesive particles. While these data can be analyzed to identify trends of decreasing fraction of organic carbon (fOC) with increasing non-cohesive fraction or with increasing bulk density (an indicator of increasing non-cohesive fraction), it is not possible to discern whether the measured fOC is associated with the cohesive sediment mixed in with the larger particles or with the larger non-cohesive particles themselves.<br><br>In the Housatonic River, sediments were fractionated by size and fOC measured on the subsamples (Weston 2004a). In the portion of the river characterized by coarse sediments, fOC directly measured on non-cohesive particles averaged approximately 0.3%. Scanning electron microscopy analyses of Housatonic River quartz particles showed only blotchy organic films or coatings (Weston 2004b). In the muddier portions of the Housatonic River, the fOC of the larger particles was over 10% in many cases; however, this was attributed to large pieces of organic matter, including sticks and leaves. Sediment profile images (Germano & Associates 2005) of the LPR confirm the presence of macro-organic material, but this material typically has lower particle densities and behaves differently than the large sand particles represented in the model.<br><br>Table 1 in Carroll et al. (1994) reports the fOC of particles of different sizes in the Hudson River, including fOC values of 6% and more for particles greater than 293 µm. It is unlikely that these were sand particles with OC coatings based on the findings of other studies. Di Toro et al. (1991) summarize data from Prahl (1982), including measurements of fOC for sand-sized particles, which were further segregated based on density. Sand-sized particles with densities greater than 1.9 grams per cubic centimeter (g/cm3) exhibited fOC values between 0.2% and 0.4%, while the fOC values of less dense sand-sized particles exceeded 10%. It would be inconsistent to model the transport of low-density, high-fOC particles using the non-cohesive transport equations because these lower-density particles would not be transported as bedload in the same way as the sands that are represented in the LPR non-cohesive solids classes.<br><br>The vast majority of non-cohesive transport in the LPR is bedload rather than suspended load, and adding bedload to the CFT model would require additional model development and necessitate smaller time steps due to the faster settling velocity of non-cohesive particles. | Region 2's comments about the foc content of non-cohesive solids are noted, though the CPG has found that published studies document a greater range of organic matter association with sand-sized particles in estuarine and marine systems.<br><br>The underestimation of surface bed contaminant concentrations was found to correlate with areas where the ST model predicted a shift in bed composition to coarser solids. As agreed during the June 28, 2016 modeling meeting with Region 2, the model performance in this regard will be reassessed once other updates to the CFT model (including revised COPC mapping) and the HD/ST/OC models have been incorporated, to determine the potential importance of non-cohesive solids to contaminant transport predictions. The CPG will also examine areas with significant sand accumulation in the ST model, considering the predicted bed composition at the end of WY2010 and its compatibility with the COPC mapping, as suggested by Region 2 during the June 28 modeling meeting. The joint decision following the December 16, 2016 meeting to make the ST model's bed above RM 14.7 hard bottom (see Comment 552) may reduce the predicted downstream coarsening and help alleviate the noted underestimation of contaminant concentrations. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: CFT Model<br>Sub-Categories: Partitioning |

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| | | | | Based on a comparison of Figure 7-2c to Figure 7-2b, the CPG predicts that the gross deposition fluxes for non-cohesive solids (Figure 7-2c) are significantly larger than for cohesive solids (Figure 7-2b), but the net deposition fluxes do not show the same trend. The transport of solids through the system indicated by the fluxes between reaches is dominated by cohesive solids.<br><br>The CPG should consider if other issues such as dramatic changes in composition, or unrealistic partitioning assumptions, may be producing this issue. If transport associated with non-cohesive solids is significant, as stated in the footnote, the CPG should consider how to incorporate partitioning to sands into the model without requiring unacceptably long simulation times, and provide the proposed model changes to EPA for review. | | |
| 374 | Section 7 | Specific | Section 7.2.2.1, page 7, first paragraph, fourth sentence | Because the study area boundary is a diagonal line across rectangular grid cells, fluxes need to be summed across cell interfaces in both the X and Y directions below RM 0.5 where the river meets Newark Bay. Although this process is not as simple as summing across the Y direction interfaces above RM 0.5, the area below RM 0.5 should not be neglected in mass balance figures. This area represents approximately 10% of the surface area of the full 17-mile LPRSA and there are a number of important processes occurring between RM 0 and 0.5 (i.e., navigation scour and deposition during storms followed by subsequent erosion). Please present mass balance results for the complete study area from Dundee dam to the boundary with Newark Bay. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 375 | Section 7 | Specific | Section 7.2.2.1, page 8, second paragraph, second sentence | Figure 7-2 does not demonstrate that the transport of non-cohesive sediments is strongly influenced by high flow events. If this is the case, the model would likely compute larger downstream advective fluxes of non-cohesive sediments corresponding to the large gross resuspension fluxes, particularly in the upstream reaches. Please revise the report and add figures to present mass balance results for a high-flow period to support the conclusion that the higher fluxes of non-cohesive sediments reflect “the | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |

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| | | | | strong influence of high flow events," or revise the text to remove this conclusion. Below RM 2 the gross erosion and deposition fluxes are of the same magnitude. Remove the word "particularly" from this sentence. | | |
| 376 | Section 7 | Specific | Section 7.2.2.1, page 8, third paragraph, third sentence | Please revise the text and figures to present mass balance results over the full LPRSA domain demonstrating variations between high- and low-flow results. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 377 | Section 7 | Specific | Section 7.2.3.1, page 9, bullets 1 through 5 | Generally, the percentages presented agree with the figures; however, a number of the values differ to an extent that cannot be explained simply by rounding. Please verify the following calculations and revise the text as needed:<br>- First bullet: per Figure 7-3b, gross tetra-CB erosion = 2.18 + 20.07 + 42.32 + 17.1 = 81.67 kilograms per year (kg/yr) and net erosion = 2.18 – 1.79 + 20.07 – 18.06 + 42.32 – 41.19 + 17.1 – 18.61 = 2.02 kg/yr, which results in a gross to net erosion ratio of 40.4 rather than 26. If navigation scour and diffusion are considered part of gross erosion, this results in a gross to net erosion ratio of 27.6 rather than 26.<br>- Second bullet: The region between RM 14 and RM 8 represents 69% of the net sediment source for tetra-CB rather than 66% ([0 + 20.07 - 18.06 + 0.06] / [0 + 2.18 - 1.79 + 0.04 + 0 + 20.07 - 18.06 + 0.06 + 0 + 42.32 - 41.19 + 0.06 + 0.81 + 17.1 - 18.61 + 0.01] = 0.69).<br>- Third bullet: The region above RM 14 accounts for 14.3% of the net tetra-CB flux rather than 12% ([0 + 2.18 - 1.79 + 0.04] / [0 + 2.18 - 1.79 + 0.04 + 0 + 20.07 - 18.06 + 0.06 + 0 + 42.32 - 41.19 + 0.06 + 0.81 + 17.1 - 18.61 + 0.01] = 0.143).<br>- Fourth bullet: The region below RM 2 captured a mass equivalent to 8.3% of the net 2,3,7,8-TCDD flux rather than 9% ([0.71 + 10.74 - 12.16 + 0.02] / [0 + 0.56 - 0.34 + 0 + 0 + 23.48 - 20.18 + 0.11 + 0 + 44.62 - 39.4 + 0.18 + 0.71 + 10.74 - 12.16 + 0.02] = -0.083).<br>- Fifth bullet: The loading from the Upper Passaic River at Dundee Dam represents 76% of the net tetra-CB flux rather than 73% (2.29 / [0 + 2.18 - 1.79 + 0.04 + 0 + 20.07 - 18.06 + 0.06 + 0 + 42.32 - 41.19 + 0.06 + 0.81 + 17.1 - 18.61 + 0.01] = 0.76).<br>Please verify that the numbers presented in the text are consistent with Figure 7-3 and clarify whether navigation scour and diffusion are considered part of gross erosion. | The navigation scour was considered part of the gross erosion; the diffusion flux was not.<br><br>The noted discrepancies will be addressed when updating the text and figures to reflect the results of the revised model calibration. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 378 | Section 7 | Specific | Section 7.2.3.1, page 10, first paragraph after bullets | Please add RM 0 to the discussion in the text and to Figure 7-4. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text. |

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| 379 | Section 7 | Specific | Section 7.2.3.2, pages 11 through 12; Figure 7-6 | Appendix O, Figure 4-3 shows a low bias in predicted concentrations between a factor of 2 and 10 for half of the depositional cells. This bias strongly influences the results presented in this section. As an example, for the areas below RM 1 or above RM 7, the 1995 concentrations are based upon approximately 2010 data (Appendix O, Section 3.1.1.5). Therefore, outside of the RM 1-7 reach, the contaminant model results in 2010 should look like the results in 1995, and the model results in 1995 may not appropriately reflect 1995 conditions. Because the entire RM 8-17 reach falls into this category, the last two sets of bars on the bottom panels of Figures 7-5a and 7-5b, as well as the points falling far above or below the 1:1 line on the bottom panels of Figure 7-6, indicate two possibilities: either the 1995 initial conditions for those cells are off by up to four orders of magnitude, or the rate of change is over predicted . If this error exists in the RM 8-17 reach it is likely that impacts results throughout the model. The second paragraph on page 12 states that the model predicts exaggerated recovery in some cells, but does not recognize the importance of that exaggeration with respect to remedy simulation, particularly the simulation of remedies that are dependent on the model's predictive capabilities at grid cell and smaller scales. The over-prediction of recovery in depositional areas is likely the result of an under-prediction of contaminant erosion and transport from other areas. This discussion needs to be revised once the model is corrected to address underestimation of concentrations in depositional areas.<br><br>Please label the 1:1 line on Figure 7-6. | Region 2's comment about bed concentrations for RM 8-17 is noted. As discussed with Region 2 during the June 28, 2016 modeling meeting, the model performance in depositional areas will be reassessed once other updates to the CFT model and the HD/ST/OC models have been incorporated (see also Response to Comment 373). The CPG will consider whether that assessment suggests the need for altering the approach to specify the 1995 bed initial condition between RM 8-17, which is uncertain due to the lack of historical data.<br><br>The requested figure change will be made. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: CFT Model<br>Sub-Categories: Long Term Calibration |
| 380 | Section 7 | Specific | Section 7.2.3.3, page 12; Figure 7-7. | Please add RM 0 to the discussion in the text and to Figure 7-7. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |

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| 381 | Section 7 | Specific | Section 7.2.3.3, pages 12 through 13 | The discussion in this section highlights the significant impact that the fluff layer has on the CFT model results. The CFT model fluff layer is not based on the ST model fluff layer, and is used as a tuning parameter to match predicted water column concentrations with the CWCM data without any constraint on the vertical variation in concentration between the bottom layer of the water column and the 15-cm average sediment concentration. There are water column data and 15 cm sediment data, but only very limited data that were collected at shallower intervals than 15 cm. The eight thinly sliced cores collected during the 2008 LRC program do not show a consistent relationship between the near surface slice and 15-cm average. There are no data to indicate the relationship between the concentration in the fluff layer and the concentration in the top 1 to 2 cm of the sediment. The CFT fluff layer model predictions are then presented as a demonstration of the system response under high flow conditions without any way to verify that response with data. Figure 7-8 should present water column results for the surface and bottom layer in addition to the fluff layer and bed results. Concerns with the CFT fluff layer implementation are discussed further in **Comment Nos. 405 and 538 through 544**. Model behavior under high flow conditions should be revisited after concerns with the CPG parameterization of partitioning and the fluff layer are addressed. | The CFT model fluff layer algorithm has been discussed with Region 2 during the HD/ST/OC/CFT meetings cited in the Response to Comment 361. Region 2 has agreed to accept the CFT fluff layer algorithm as a reasonable approach to simulate recently deposited solids "going up and down" over a tidal cycle in a manner that mimics the sediment transport model (see Responses to Comments 405 and 545). The specific implementation concerns raised in Comments 538 to 544 have also been discussed and resolved in principle, as summarized in the responses to those comments.<br><br>The requested addition to Figure 7-8 will be made, and the high flow behavior of the model will be revisited once the other model changes are implemented.<br><br>The CPG notes that the eight thinly sliced cores collected during the 2008 LPR program do suggest a structure of the mean concentration gradient over the top 15 cm sediments for 2,3,7,8-TCDD, and a qualitatively similar gradient of the mean concentration is captured by the CFT model. This model-data comparison will be added to the report.<br><br>It is also noted that, in the CPG's opinion, the contaminant concentrations in the CFT fluff layer are indirectly constrained because they mediate the COPC transfer between the water column and the sediment, which is needed to replicate the CWCM data and the long-term contaminant trajectory in sediments. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>It is noted that the statement about the 2008 thinly sliced cores is not supported by the near-surface data from those cores. Half of the cores have higher top 2 cm 2,3,7,8-TCDD concentrations and half have higher 15 cm average 2,3,7,8-TCDD concentrations.<br><br>Although the fluff layer and surface sediment concentrations can be anticipated to fall somewhere between the measured water column and measured 15 cm sediment concentrations. The shape of the gradient from 15 cm sediment to surface sediment to fluff to water column concentrations is not constrained by the available data.<br><br>Categories: Model Consistency, CFT Model<br>Sub-Categories: Fluff Layer |
| 382 | Section 7 | Specific | Section 7.2.3.3, page 13, third paragraph, second sentence | The text discusses the water column response to Hurricane Irene, but the water column model results are not presented. Please add the water column predicted particulate concentrations to Figure 7-8 and present predicted total water column results on a volumetric basis for the same period. | The requested change will be made in Figure 7-8 or in a new figure. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 383 | Section 7 | Specific | Section 7.2.4.1, page 14, second paragraph, first sentence | Figures 7-9 and 7-10 underestimate the role of sediments within the food web due to issues with the bioaccumulation model calibration. As shown in the review below in **Comment No. 614**, the assumption that benthic organism biomass is dominated by deposit feeders is flawed due to non-site-specific organism weights used. This has implications for predator feeding preferences as they were ostensibly based on benthic organism biomasses. After the model is recalibrated, these figures will need to be reproduced. | A literature search will be conducted to identify additional relevant benthic biomass data. Benthic organism biomass will be recalculated to: 1) include any new biomass data that are found during the literature review; and 2) omit Corbicula shell weights. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Benthos |

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| 384 | Section 7 | Specific | Section 7.2.4.1, page 14, third paragraph, last sentence | Because the two groups of fish will respond differently to remediation, it is important that their relative abundance is properly considered within the bioaccumulation model. Based on abundance data, filter-feeding fish are much less prevalent in the LPRSA than small forage fish, but this is not reflected in feeding preferences of higher predators. As directed below in **Comment No. 585**, feeding preferences must be reconfigured in the bioaccumulation model to properly reflect small fish abundance data. | Abundance data for small fish in the Passaic will be evaluated, and a discussion of the results will be included in the revised report. Abundance data will be considered along with other relevant factors when determining feeding preferences of predators of small fish. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Feeding Preference |
| 385 | Appendix K | General | | The "Alternative OC model" is referred to as the "OC model" in Appendix K, but in Appendix N the model is referred to as the "AOC model." Please revise the RI Report and appendices as necessary to ensure that the term "OC model" is used consistently and remove all use of the term "AOC model." | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 386 | Appendix K | Specific | Figures 13, 14, 15, 17, 18 and 19 | The combination of CPG models appear to predict a more rapid "natural recovery" in strongly depositional areas than is suggested by the data presented in Figures 13, 14, 15, 17, 18 and 19. The underlying factors that contribute to this miscalibration need to be investigated and corrected prior to use as a management tool. | As discussed with Region 2 during the June 28, 2016 modeling meeting, the CPG will re-evaluate model-predicted recovery in depositional areas after implementing all other updates to the ST, OC, and CFT. See also Response to Comment 373. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: CFT Model<br>Sub-Categories: Calibration |
| 387 | Appendix K | Specific | Section 2.2.1, page 3, first paragraph, first sentence (continued from page 2) | Please revise the text to state that the grid is mostly four grid cells across in the lower miles, with a maximum of six across for a short stretch in the Harrison Reach. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 388 | Appendix K | Specific | Section 3.1.1, page 8, second paragraph, third and fourth sentences | Please revise the text to include a statement regarding the limited amount of data above 2,000 cfs. | Please note that the Spring PWCM and the Sommerfield and Chant 2008 to 2009 deployment include data at flows greater than 2,000 cfs. | Please provide a quantitative description of the fraction of data collected on days with daily average flows greater than 2000 cfs.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 389 | Appendix K | Specific | Section 3.1.1, page 9, second paragraph, second sentence, Section 3.3.1, page 12, first paragraph, first sentence | The reference to Hurricane Irene as a "100-year event" is inconsistent with the RI Report, Section 7.1 (page 3, first paragraph, second complete sentence), which refers to Hurricane Irene as a "1-in-90 year storm event." Characterization of Hurricane Irene as a 90-year event is consistent with the USGS statistics for the storm (refer to http://nj.usgs.gov/hazards/flood/flood1108/ and http://nj.usgs.gov/hazards/flood/flood1108/docs/gagepeaksummaryaug27-30.pdf Page 2, Station 01389500, Passaic River at Little Falls, Recurrence Interval = 90). Please revise the text to be consistent with Section 7.1 of the RI Report and with USGS statistics. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |

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| 390 | Appendix K | Specific | Section 3.1.1, page 9, second paragraph, third sentence | Please revise the text to clarify that the erosion near the mouth of the Passaic River during low-flow periods is due to a combination of navigation scour and tidal resuspension. Refer to **Comment No. 368** concerning the description of navigation scour in RI Section 7.1. | Shear stresses in areas exhibiting navigation scour at the mouth of the LPR under low-flow/normal-tidal conditions are lower than during high-flow conditions. Therefore, if sediments deposit in these areas during high-flow conditions, it is highly unlikely that these areas will exhibit erosion during subsequent low-flow/normal-tidal conditions due to tidal currents alone. See Response to Comment 474 for a detailed explanation of why vessel navigation is the only mechanism that can explain the observed scour. | The text should be revised to include a discussion of additional potential factors (e.g. stresses due to storm surges) and clearly state the basis for the suggested more-likely cause of erosion near the mouth of the LPR during low flow conditions.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications, Navigation Scour |
| 391 | Appendix K | Specific | Section 3.1.2, page 9, first paragraph, second sentence | Please revise the text to reflect that although the CPG RI and EPA FFS models were initially identical, both sets of code have been modified. It is true that many of the modifications made to either version of the model have been incorporated into both (e.g., the Sanford consolidation model, the fluff layer representation), however the current versions of the EPA FFS and CPG models are not identical. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 392 | Appendix K | Specific | Section 3.2.2, page 11, first paragraph, fifth sentence | Please clarify the way in which the critical shear stresses were defined for the silts and clays. In addition, this paragraph should be broken into multiple paragraphs for ease of reading. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document, HST Model<br>Sub-Categories: Text Clarifications, Erosion |
| 393 | Appendix K | Specific | Section 3.2.2, page 11, last sentence, and page 12, first sentence | Please revise the text to clarify how the grain size and dry density data were interpolated onto the grid. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 394 | Appendix K | Specific | Section 3.3.1, page 13, first paragraph, second and third sentences | Please clarify if the calibration of the settling velocities was revised after the calibration of critical shear stresses for erosion from the parent bed or if the calibrations were truly “mutually exclusive” as stated in the text. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 395 | Appendix K | Specific | Section 3.3.2.3, page 14 | Please revise the text to acknowledge that at RM 1.4, the model predicts that net upstream transport will continue at higher flows than the data indicate, and that at RM 6.7, less upstream transport is calculated as compared to the data. | The data are subject to several sources of uncertainty: SSC is estimated from ABS (and subject to associated variability in the ABS-SSC regression); SSC profile estimated from ABS needs extrapolation to the surface/bottom of the water column (this is also an issue for velocity); SSC is a point measurement but model results represent an average across the cell width (this is also an issue for velocity). Therefore, there are several issues affecting certitude in the data, and which imply that we should not look for a precise correspondence between model and data. The CPG will update the text to include this description of uncertainty in the data which has some impact on the model-data comparisons. | When the text is edited to describe the revised modeling results (as presented by the CPG modeling team on March 29th 2017) the comparisons between model results and sediment fluxes derived from the ABS data should be presented with a balanced tone, and should include a discussion of the uncertainty of estimating fluxes from the ABS and ADCP data.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |

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| 396 | Appendix K | Specific | Section 3.3.2.4, page 15 | In the parenthetical phrase that refers to 50% of the 2008-2010 erosion reflecting sub-grid scale localized scour, clarify if the comment applies to erosion in each reach or only in the RM 14-2 reach. Also clarify if the term "and resulting deposition" refers to deposition in the RM 2-0 reach due to localized scour from the RM 14-2 reach. The text overstates agreement between the model and data. The fate of the additional erosion volume from RM 2-14 over 2007-2012 period should be discussed. In addition, the text should summarize results on a gross and net basis for the total area plus the erosional and depositional areas individually. Finally, this section should be broken into multiple paragraphs for readability. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Bathymetry, Text Clarifications |
| 397 | Appendix K | Specific | Section 4.2, page 17, second paragraph, first and second sentences | Refer to **Comment Nos. 532 and 533** related to the $f_{OC}$ bulk density relationship. A power function may provide a better fit of the $f_{OC}$ data than the linear regression that was used. In addition, the text in this section states that the bulk density presented is dry bulk density but that is not stated in Appendix N where this relationship is presented again. In addition to the linear regression presented in the text, alternative regression forms should be tested for fitting the $f_{OC}$ dry bulk density relationship. | Alternative foc dry bulk density relationships will be explored and the corresponding revisions will be made to the text.<br><br>The text will be revised to clearly indicate that "bulk density" refers to "dry bulk density" unless otherwise noted, as is common practice. | The response is accepted pending review and approval of the revisions to the $f_{OC,}$ bulk density relationship and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: OC Model<br>Sub-Categories: Bed $f_{OC}$ |
| 398 | Appendix K | Specific | Section 4.2, page 17, third paragraph | It is unclear why DOC is mentioned in the OC model description. Based on the approach used by the CPG the water column DOC in the CPG's models is a constant value in both time and space, and is not modeled or used in any of its OC, CFT, or bioaccumulation calculations. EPA did not derive a spatially and temporally constant water column DOC value as suggested by the text, although this was done for the sediment. The CPG must represent water column DOC partitioning in the CFT model and its impacts on bioavailability in their bioaccumulation model. The CPG should perform an analysis of the New Jersey Harbor Dischargers Group DOC data to determine if seasonal or spatial trends exist in the data and represent those trends in the CFT model input. The CPGs RI text will need to be revised once these corrections are made to the model. | The requested changes in the OC model text will be made. | Please clarify if the CPG will evaluate the spatial and temporal variation in DOC and incorporate the results of that evaluation into the contaminant model.<br><br>Categories: RI Document, CFT Model<br>Sub-Categories: Text Clarifications, Partitioning |
| 399 | Appendix K | Specific | Section 4.3, pages 17 through 18 | The comparison of the OC model results to ST-SWEM results was not done using the OC model as implemented in the RI. As noted in **Comment No. 534**, longer test simulations should be conducted using the model as implemented in the RI and compared to the available water column chlorophyll-*a* and particulate organic carbon (POC) data. | As discussed with Region 2 during the June 28, 2016 modeling meeting, the CPG agreed to implement the mass balance approach as is currently presented in the RI. However, the CPG also stated that it will switch back to the constant foc approach if the mass balance approach does not provide reasonable carbon estimates, with the findings documented in the revised RI Report. The requested comparison will be conducted (see also Response to Comment 518). | If the mass balance approach does not work, the reasons for the imbalance should be explored, identified and addressed.<br><br>Categories: OC Model<br>Sub-Categories: Mass Balance |
| 400 | Appendix K | Specific | Figures 9a through 13b | Please revise these figures to plot all contaminant concentration panels (volumetric or solids-normalized) using consistent scales on the y-axis. Using different scales tends to mask the spatial similarity in the observed data across stations, events, depths and flow conditions. In addition, please print the model median and data median concentrations on the plots, and include a label for the x-axis of the panels. | The requested change on the figures will be made, or alternate versions of the figures will be included that satisfy the request | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures |

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| 401 | Appendix K | Specific | Figures 15a and 15b | The CFT model is biased slightly high with respect to the 2010 TCDD concentrations in the erosional areas and biased low with respect to the 2010 tetra-CB concentrations in the erosional areas, but the model is biased low for both TCDD and tetra-CB in the strongly depositional areas. This might result in an overestimation of the rate of recovery. Please correct the model to address this bias and revise the text accordingly. | See Response to Comment 373. The text will be revised accordingly. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: CFT Model<br>Sub-Categories: Calibration |
| 402 | Appendix K | Specific | Section 5.2, pages 20 through 23 | Please revise the text regarding the model application to the LPR based on the discussion of these topics in **Comment Nos. 535 through 565** on Appendix O. | The text will be revised accordingly to reflect the CPG Responses to Comments 535 through 565. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 403 | Appendix K | Specific | Section 5.3.1, page 23, third sentence | The text implies that it is inappropriate to test the model's performance based on spatially and temporally paired values for the water column and spatially paired values for the sediment. Please clarify why these paired values are used in the comparison of model output and data distributions. Refer to the description of pairing in Appendix O, Section 4.1.1. Given the small footprint of the CPG's previously proposed remedial alternative, it is important that the model appropriately represent both the temporal and spatial behavior of contaminant transport within the LPR. | In the draft RI Report, the space-time coordinates of the data were used to choose model grid elements and time periods to extract model results to contrast with the data. These model results and data were not compared one to one, rather the distribution of values from the model were compared to the distribution of data values (Figures 9, 11, and 12 for the water column, and Figure 13 for surface sediments). The selection of model values in this manner was done to best "sample" the range of conditions that are comparable to the data. This effort at matching model and data was done to take account of large scale trends that could corrupt blind comparisons of all the model grid elements to the more spatially and temporally restricted data. The CPG feels that these metrics, coupled with the other model-data comparisons provided in Section 5.3, provide a reasonable assessment of the model's ability to replicate the spatial patterns and temporal dynamics of the contaminant data. Reproducing these behaviors in the data is evidence that the model processes are constrained and sufficiently realistic to support the evaluation of remedial alternatives, provided that they are also realistically represented (see Response to Comments 404 and 547).<br><br>The alternate approach of characterizing model to data agreement on a one-to-one basis (e.g., crossplots of predicted versus observed for each paired set) was not used because of concerns that it places more emphasis on the precision of space-time matching of model-to-data than is warranted by the nature of the datasets and the model resolution. A few such considerations are noted below:<br>• For the sediment bed, the 1995 and 2010 datasets are not co-located. There are no measurements of concentration changes at individual locations and the | The report should be modified to include the requested comparisons and quantitative calibration metrics for the complete set of paired values. The text may be revised to reflect the limitations the CPG has noted.<br><br>In the case of the sediment concentrations, some understanding of the model's ability to reproduce the measured spatial patterns of contaminant is necessary. If the remedial footprint and post remedy concentrations in a given cell are based upon data that are at odds with the model predicted concentration at the start of remediation in that same cell, the result may be either an over or underestimate of the remedial benefit predicted in the FS.<br><br>Given the estuarine transport processes in the LPR, the timing of, and spatial and vertical distribution of contaminant responses to tidal resuspension will impact the direction and distance those contaminants travel. Simply looking at distributions does not provide enough information to assess that these processes are captured correctly.<br><br>Categories: CFT Model<br>Sub-Categories: Calibration Metrics |

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| | | | | | number of measurements within single model grid cells is small so individual grid cell changes are subject to considerable uncertainty. Given the data density and lack of pairing, meaningful comparisons can only be made for groups of grid cells. Paired model-to-data comparisons reflect the influence of 1995 initial conditions that are not likely to be accurate at the scale of the 2010 target data points, which can yield a poor agreement even if the model is producing realistic contaminant fluxes. This limitation is de-emphasized by conducting the comparison on a distributional basis, allowing the representativeness of the predicted concentrations to be judged on a spatial scale that balances data availability with spatial resolution needs (e.g., as driven by the bioaccumulation model's segmentation).<br>• For the water column, model to data matching is limited in space by the horizontal and vertical resolution of the grid and in time by the temporal resolution of the output and the precision of the model in reproducing the exact timing of fluxes. For example, a mismatch in the timing of peak flood/ebb could yield poor model-data agreement on a paired basis even if the net flux and concentration range over the tidal cycle was well-predicted. Such limitations are de-emphasized by evaluating the model's ability to predict the distribution of concentrations for the same conditions as those sampled, which is more relevant considering the long timescales of FS evaluations and the time- and space-averaged concentrations used by the bioaccumulation model.<br><br>Nevertheless, the CPG will consider including additional quantitative and/or qualitative metrics to assess model performance on a finer spatial and temporal scale, in response to this comment as well as Comment 561. For example, temporal figures of predicted and observed near-bottom water column contaminant concentrations on intra-tidal time scales will be added (similar to the 2,3,7,8-TCDD figures transmitted to Region 2 on September 18, 2016). Further details regarding the calibration metrics will be | |

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| | | | | | discussed during the ongoing modeling interactions with Region 2.<br><br>The text will also be revised to clarify data usage in the calibration. | |
| 404 | Appendix K | Specific | Section 5.3.1, page 23, fifth sentence | If it is "beyond the capability of the model to resolve sub-grid scale phenomena," then the model cannot be used in the FS to assess the benefits of remedial alternatives applied at scales finer than the grid cells. | The representation of remedial benefit for a targeted remedy has been discussed as part of recent interactions with Region 2 on COPC mapping, most recently at the April 27, 2016 mapping meeting. The CPG presented its proposed approach to translate remediation at the scale of the contaminant maps to the scale of the model grid. The latter step allows the model to realistically account for the expected scale of the design sampling and target delineation in addition to the contaminant data upon which the model's initial condition is based. The proposal is under Region 2 review, and the final approach is pending further feedback from Region 2.<br><br>The CPG does not agree that calculation of grid scale concentration changes resulting from mapping-based delineation that is finer than grid scale requires understanding sub-grid scale fate processes. Given the approximate nature of FS-level alternatives and the use of grid scale initial conditions based on sub-grid scale mapping, the CPG believes its proposed approach is valid. It is conceptually similar to those used at other sites, such as the Grasse River and Hudson River. | See response to Comment 403. The issue of remedial benefit calculations was discussed at the March 29, 2017 meeting, and requires further follow up to resolve concerns with the CPG's proposed approach.<br><br>Categories: CFT Model<br>Sub-Categories: Remedial Benefit |

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| 405 | Appendix K | Specific | Section 5.3.1, page 23, last sentence (continued on page 24) | The application of the fluff layer particle mixing must be corrected (refer to Appendix O, **Comment No. 542**) and the value constrained to be greater than or equal to the Layer 1-2 mixing. This would be consistent with the vertical structure of mixing the CPG has proposed as well as their proposed feeding structure dominated by organisms residing at the surface of the sediment and feeding from the fluff layer. The fluff layer thickness and its transfer to the bed should be consistent with the ST model. Please correct these inconsistencies. | Regarding updates to be implemented in specifying the exchange between the fluff layer and the underlying sediment layer, please see Response to Comment 542.<br><br>Regarding the magnitude of the mass transfer rate between the fluff and the underlying layer, the CPG disagrees this quantity should be restricted to values equal to or greater than the mixing rate between the underlying bed layers 1 and 2, as discussed with Region 2 at the June 28, 2016 modeling meeting. The biological and physical processes accounting for exchange between the fluff layer and the underlying layer are not explicated modeled, are not well understood, and may differ in magnitude and function from mixing in the underlying consolidated bed. The CPG proposal is that the mass transfer be modeled on a total chemical basis (as noted in Response to Comment 542) with the transfer rate to be determined by calibration. Interpretation of the calibrated values will be included in the revised text and/or discussed with Region 2 when reviewing revised model results. This approach was discussed at the December 16, 2016 modeling meeting and January 6, 2017 follow-up call.<br><br>Regarding the model's representation of fluff layer thickness, it was agreed at the September 20, 2016 modeling meeting (as previously noted in the Response to Comment 381) that the present fluff layer tracking approach could be maintained as a reasonable approach to simulate recently deposited solids "going up and down" over the tidal cycle, which overcomes the inherent difficulties and pitfalls of directly adopting the ST model's definition of the fluff layer within the CFT model. To support this discussion, the CPG provided notes on the conceptual differences between the ST and CFT model fluff layer algorithms (June 27, 2016), and presented at the September meeting detailed diagnostics of ST and CFT model predictions to demonstrate the CFT model's ability to reproduce the ST model's intratidal thickness variations. Included in this discussion was the role of the thickness transfer term in allowing the CFT fluff to adapt to changing shear stress conditions without needing to deconstruct the ST model's complex layer tracking scheme (see the Response to Comment 545 for additional detail). Thus, the fluff layer thickness tracking, including the thickness transfer to the bed, will be maintained in the revised model. | The CPG can continue with the proposed thickness transfer approach but will need to set the mixing rate between the fluff layer and layer 1 of the bed equal to or greater than the mixing between layer 1 and layer 2 of the bed. The CPG has commented that the majority of the benthos are feeding at the interface between the fluff and layer 1 of the bed, that is the place where the greatest mechanical mixing occurs, and that is where the porosity is greatest and bulk density the lowest, all of which would contribute to greater mixing at the surface of the sediment.<br><br>Categories: CFT Model<br>Sub-Categories: Fluff Layer |

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| 406 | Appendix K | Specific | Section 5.3.2.2, page 25, first sentence | Please clarify the statement that the model provides "a reasonable vertical concentration profile." There are no vertical data profiles available against which to compare the model; the computed vertical profile simply fits the assumptions made by the CPG modeling team. | The requested clarification will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Figures |
| 407 | Appendix K | Specific | Section 5.3.2.2, page 27, second paragraph, sixth and seventh sentences | While assessing the model to data comparison on a cohesive solids basis does provide some improvement in model skill, the model is still biased low in depositional areas both on a cohesive solids basis and an OC-normalized basis. Expand the analysis to separate the depositional results into the mildly and strongly depositional categories presented in Figure 15. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 408 | Appendix K | Specific | Section 6.2.1, page 31, second paragraph after first set of bullets, first sentence | Model calibration focused on model performance while averaging all data over the modeling areas as a single zero-dimensional (0D) segment is not appropriate, as discussed in **Comment Nos. 574, 577, 579, 594, 597, 600, and 601** below. There are significant differences by RM in sediment concentrations, fish tissue concentrations, salinity, oxygen, and organic matter content, among other factors. The model performance must be examined at a finer spatial scale by creating model to data comparisons by RM bins. | The bioaccumulation model will be partitioned and calibrated for estuarine, transitional, and freshwater reaches. The transitional reach might be further subdivided, if warranted based on changes in food web structure across the transitional reach. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration, Spatial Variability |
| 409 | Appendix K | Specific | Section 6.3.2, page 33, first sentence | By focusing the model calibration on model performance for six target species, the CPG has eliminated a large set of data from their model performance testing, especially forage fish (n=4), mummichog (n=18), perch (n=22), and benthic invertebrates (n=19). EPA directs the CPG to include these species in the model performance testing.<br><br>The calibrated model performance is reasonable based on the limited metrics presented. However, the breadth of metrics presented is not wide enough to demonstrate the model's performance. As directed below in Comment No. 609, the CPG will need to recalibrate the bioaccumulation model. | The CPG agrees that using as much data as possible to calibrate the model is ideal. The existing model calibration incorporates white perch. However, because of concerns regarding the representativeness of the available benthic chemistry and small fish data, these data were not considered suitable for inclusion as part of the calibration. The uncertainties associated with these data will be better documented in the revised report. | EPA recognizes that small sample sizes increase uncertainty and may reduce representativeness. However, there are methods of incorporating the sample size into model performance metrics (weighting higher-sample-size organisms more heavily, for example).<br><br>Even though some of these organisms have lower sample sizes, this line of evidence should not be removed from the model calibration assessment.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration Targets |
| 410 | Appendix L | General | | Figures should be described in sufficient detail within the text for the reader to easily understand the content of each figure, and sufficient information should be provided on each figure for the reader to easily comprehend all the information being presented (e.g., Section 2.2.1 and Figure 2-10). In addition, units displayed in legends and on figures should be consistent throughout (e.g., no units on Figure 2-3 and 2-4 and meters on Figure 2-5). Please revise the text and figures to ensure that the information presented is clear and consistent. All time series plots should be illustrated consistently (e.g., colors used for model and data on Figure 2-35 and 2-38), and line widths selected so that modeled and measured data can be easily identified (e.g., Figure 2-21). | The text will be expanded with additional description of the figures. Figures will be reviewed to ensure the legend/units are shown. Certain figures which show up with a poor resolution in the Word document (e.g., Figure 2-25) were inserted as EPS files, which shows up on screen with a poor resolution when viewing the Word document but when zoomed in (in Word), printed to hard-copy, or saved as PDF, display with good resolution. These figures are not planned to be updated since model results and data are legible in the final mode of publication—hard copy/PDF. Consistency in colors used for model and data across figures will be ensured only in cases where the various figures are being cross-compared in the text. | The clarity of figures in PDF and printed versions has been confirmed. The response is accepted.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |

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| 411 | Appendix L | General | | Please revise the text to avoid the use of vague statements such as: "These references were not eliminated in the restart files, but <u>other things were changed within this file</u>" (Section 2.2.6, page 17, first paragraph, last sentence), "The format for the input files of the heat flux conditions was changes from the calibration runs" (Section 2.2.5) "changes were made to the format of gcmtsr, gcmplt and the restart files" (Section 2.2.6) and "Post-processing tools needed to be modified to meet the new requirements" (Section 2.2.6).<br><br>The text should either include details about what was changed, if relevant, or state that the changes were not relevant. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 412 | Appendix L | General | | Figures presented in landscape orientation (e.g. Figures 6-1 – 6.5) should have captions in landscape orientation. | The requested change will be made. | Response accepted, pending review of the revised figures.<br><br>Categories: RI Document<br>Sub-Categories: Figures |
| 413 | Appendix L | Specific | Section 2.1.2, page 12, third paragraph, fifth and sixth sentences, Section 2.2.1, page 16, fourth and fifth sentences | Please renumber and reorder the figures as necessary or revise the text so that the figures are sequential in the text and figure sheets. In this section, Figures 2-14 and 2-15 are called out before Figures 2-9 through 2-13, which are not referenced until later sections. Figure 2-11 is called out before Figure 2-10. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 414 | Appendix L | Specific | Section 2.1.7, page 15 | Please revise the text to identify where the Tierra Solutions, Inc. (TSI) instruments were located during deployment and where the shipboard transect measurements were completed. References to the corresponding reports should be included. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 415 | Appendix L | Specific | Section 2.1.8, page 15 | Please revise the text to identify where the Rutgers instruments were located between 2000 and 2002. References to the corresponding reports should be included. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 416 | Appendix L | Specific | Section 2.1.9, page 15 | Please revise the text to identify where the Rutgers instruments were located in 2004. References to the corresponding reports should be included. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 417 | Appendix L | Specific | Section 2.2, page 15, second paragraph, first sentence | Please revise the text to clarify whether the referenced modifications to the ECOM code are those made by HDR, Inc. (HDR) before the code was passed along to the CPG, or if additional modifications were made by the CPG. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 418 | Appendix L | Specific | Section 2.2, page 16, first paragraph, | Please add the missing word "removed" between the words "was" and "entirely." | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |

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| | | | fourth complete sentence | | | |
| 419 | Appendix L | Specific | Section 2.2.1, page 16, last sentence | Please clarify the statement that "The changes were designed to "straighten" the Hackensack as the turning of the model was no longer required to increase computational time." | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 420 | Appendix L | Specific | Section 2.2.5, page 17 | Please augment this section with additional information regarding the changes from the calibration runs. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 421 | Appendix L | Specific | Section 2.2.6, page 17, second paragraph, fourth sentence | Please clarify the purpose of the statement that "There is no clear description of which variables are time dependent, and which are purely initial conditions for the hot started model." It is unclear whether this description is needed, or if the intent is to establish a record of what was/was not provided to the CPG. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 422 | Appendix L | Specific | Section 2.2.6, page 17 | The discussion in this section suggests that there were changes made to the model code and input files that were not adequately explained to the CPG or given to the CPG. Please clarify whether this is the case and, if so, whether this information has been requested by the CPG. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 423 | Appendix L | Specific | Section 2.3, pages 18 through 20 | Please revise this section to include discussion of the model performance metrics. Quantitative statistical comparisons should be computed on the model-data performance to provide a metric other than qualitative description of agreement. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document, HST Model<br>Sub-Categories: Text Clarifications, Calibration Metrics |
| 424 | Appendix L | Specific | Section 2.3, pages 18 through 20 | Some of the figures referenced in this section include data that appear suspect (e.g., spikes in velocity on the top panel of Figure 2-22). Please revise this section to indicate whether there was an attempt to filter suspect data and if so, provide a description of the process by which unreliable data were vetted and filtered and the criteria applied to determine when and when not to use these data. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 425 | Appendix L | Specific | Section 2.3, page 19, last paragraph, third sentence | Please revise the text to clarify whether "the dry case" refers to the very low flow case. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 426 | Appendix L | Specific | Section 2.3, page 20, fourth paragraph, fourth sentence | Please revise the text to include more description to fully define the information plotted on all subplots in Figure 2-38. For instance, U*S should be defined in the text and on the figure. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 427 | Appendix L | Specific | Figure 2-3, page 24 and Figure 2-4, page 25 | Text on page 12 describes Figures 2-3 and 2-4 as showing bathymetry used in the model, however there appear to be some blank (white) cells in the figure. Please modify the figures to present the bathymetry for these cells. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text. |

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| | | | | Also, please explain how data were treated if coverage of a grid cell was less than 100%. In addition, please add units to the legends. | | Categories: RI Document<br>Sub-Categories: Bathymetry, Figures, Text Clarifications |
| 428 | Appendix L | Specific | Figure 2-10, page 30 | Please clarify what this figure is illustrating, including identifying which panel shows the calibration grid and which shows the 10-year run grid and labeling the axes. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 429 | Appendix L | Specific | Figure 2-14, page 34 | It appears that there might be a plotting error in the figure as the coloring appears in a zig-zag pattern up the river channel. Please verify and re-plot the figure if necessary. | This was not an error but an optical illusion related to data density, symbol size, and the order in which ArcGIS plotted the symbols (across the river, alternating from one side to the other). Will revise figure to try and eliminate this plotting artifact. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Bathymetry, Figures |
| 430 | Appendix L | Specific | Figure 2-20, page 39 | Please clarify whether the discharge data were obtained from the Little Falls gage or somewhere else. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 431 | Appendix L | Specific | Figure 2-26, page 44 | It appears that there might be a bias in the way the salinity data in these figures (2-25 2-26, 2-27 and 2-35) were measured that prevented salinity from being measured at 0 ppt. If a bias is identified in the measured data and the data in the figures are shifted accordingly (vertically), the measured data may better align with the modeled salinity. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 432 | Appendix L | Specific | Figure 2-38, page 54 | It is unclear exactly what is being plotted in this figure. Please provide additional description in the text and/or the figure to clarify. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 433 | Appendix L | Specific | Section 3.2.3, page 58, third sentence | Please clarify whether the “EPA survey from 2005” is the survey completed by Tierra Solutions, Inc. in 2005. A reference should be added to the text. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Bathymetry, Text Clarifications |
| 434 | Appendix L | Specific | Figure 3-1, page 60 | The red dots shown in the figure should be included in the legend. This figure should be replaced with a larger, clearer version. | The requested change will be made. | Response accepted, pending review of the revised figures.<br><br>Categories: RI Document<br>Sub-Categories: Figures |
| 435 | Appendix L | Specific | Figure 3-2, page 61, bottom subplot | Please revise this figure to plot the wind direction as points rather than a line to avoid connecting directions on either side of the 360/0-degree threshold. | The requested change will be made. | Response accepted, pending review of the revised figures.<br><br>Categories: RI Document<br>Sub-Categories: Figures |
| 436 | Appendix L | Specific | Section 4.0, page 67, third paragraph, third sentence | Please verify the date of the “large discharge event” referenced in this sentence. It appears that the year should be 1996, not 2006. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |

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| 437 | Appendix L | Specific | Figures 4-2 and Figure 4-3, page 69 | Please label the x-axes of these figures. It is unclear whether the numbers are miles or kilometers. | The requested change will be made. | Response accepted, pending review of the revised figures.<br><br>Categories: RI Document<br>Sub-Categories: Figures |
| 438 | Appendix L | Specific | Figures 6-1 through 6-5, pages 79 through 83 | Please clarify whether any quantitative model performance metrics were computed to assess model-data agreement. Some quantitative measure of performance should be estimated and presented in these figures to show model agreement performance. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document, HST Model<br>Sub-Categories: Text Clarifications, Calibration Metrics |
| 439 | Appendix L | Specific | Section 6.4, page 94, second paragraph, last sentence, Figure 6-14 | Please clarify whether reference to "event #1" in the text refers to the panel on Figure 6-14 labeled "routine event #1". In addition, text should be included to characterize the agreement of the data and model results for high-flow event #1, which are shown on Figure 6-14, but not discussed in the text. The panel labeled "High-flow Event 1" shows that the model tended to under-predict the measured salinity concentrations for the upper end of the range of the salinity data. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 440 | Appendix L | Specific | Tables A-1 and A-2, page 99 | Please verify whether the correlation coefficients, all greater than 0.98, provided in the water level and discharge statistical comparisons are correct given the tabulated root mean square (RMS) errors. In addition, please verify that the correlation coefficients (r) in the tables are not the coefficient of determination ($r^2$). | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 441 | Appendix L | Specific | Appendix B, page 109, second sentence | This sentence is incomplete. Rather than omit these flow data, the amount of rainfall at the time should be reviewed to determine whether the flow of 35,000 cfs is consistent with the rainfall data and should be included in the flow frequency analysis. Please revise the text and frequency analysis accordingly. Alternatively the analysis should be presented with and without this value included. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 442 | Appendix M | General | | The discussion of the comparisons between model results and data focuses on instances where model results are in better agreement with data and does not provide a balanced treatment of the cases where the comparisons are not as favorable. Please revise the text to include balanced discussions of the model-data performance. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 443 | Appendix M | Specific | Section 1.0, page 12, second paragraph, last sentence | Please note the typographical error and revise "and presented" to read "are presented." | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 444 | Appendix M | Specific | Section 2.3.3, page 17, last paragraph, third sentence | Please correct the subject-verb agreement in this sentence by revising "were normalized" to read "was normalized." | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 445 | Appendix M | Specific | Section 2.3.10, page 24, last two sentences | Please revise the text to present these two sentences as a hypothesis rather than a fact. Considerable uncertainty is often evident when attempting to assess bed evolution by comparison of bathymetric surveys. While navigation scour is a possible explanation for survey differences, it has not been proven to be the cause. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |

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| 446 | Appendix M | Specific | Section 4.2.4, page 38, last paragraph, sixth sentence and Figure 20 | The text states that "Square and circle symbols are used to denote the rating curve derived estimates." Figure 20 also includes triangular symbols. Please revise the text accordingly and include the triangular symbols in the legend in Figure 20. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 447 | Appendix M | Specific | Section 4.2.4, page 40, last paragraph, and Figure 23, page 105 | Please add a notation to Figure 23 to explain what the red lines represent and include this explanation in the discussion of Figure 23 in the text. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 448 | Appendix M | Specific | Section 4.2.5, page 47, first paragraph, fourth sentence | Please revise the text to clarify whether the negative entrainment rates were excluded from the development of the functional relationship discussed in the text. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document, HST Model<br>Sub-Categories: Text Clarifications, Erosion |
| 449 | Appendix M | Specific | Section 4.2.5, page 50, last paragraph, fourth sentence | Please revise the text to provide justification for using a cutoff of >50% cohesive solids in the data screening, given that a criterion of <15% cohesive content (as reported in Section 4.3.2, page 55, second bullet) is used to distinguish non-cohesive from cohesive behavior of the active layer. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document, HST Model<br>Sub-Categories: Text Clarifications, Erosion |
| 450 | Appendix M | Specific | Section 4.3.2, page 55, third bullet | The reasonableness of using the initial D50 to calculate skin friction needs to be demonstrated by showing limited temporal variation in D50 through long-term simulations or by conducting sensitivity analyses with alternate D50 values, since the model results indicate that significant variations in D50 evolved during the calibration simulation. The text will need to be revised to provide justification for use of the initial D50 once such analysis has been performed. | At the June 28, 2016 modeling meeting, the CPG proposed not performing additional model development since comparison of the CPG model (which uses constant D50) performance to the FFS ROD model (which uses time-variable D50) shows largely similar performance, indicating very little change/improvement in model performance to be gained by implementing this feature. Subsequently, at the meeting on September 20, 2016, Region 2 indicated acceptance of the CPG's proposal. No changes will be made to the model in this regard. | Response accepted, pending review of the revised text.<br><br>Categories: HST Model<br>Sub-Categories: D50 |
| 451 | Appendix M | Specific | Section 4.3.4, page 56, last paragraph, fifth through seventh sentences | The effect (or lack thereof) of the lag should be demonstrated by repeating the calibration simulation without the lag. The text should then be revised to discuss the results of this comparison. It is recognized that this will increase the overall runtime for the demonstration simulation. | At the June 28, 2016 modeling meeting, the CPG proposed not performing additional model development since comparison of the CPG model (which includes a lag in bathymetric updates) performance to the FFS ROD model (which has no lag in bathymetric updates) shows largely similar performance, indicating very little change/improvement in model performance to be gained by implementing this feature. Subsequently, at the meeting on September 20, 2016, Region 2 indicated acceptance of the CPG's proposal. No changes will be made to the model in this regard. | Based on discussions with the CPG modeling team, the present approach for updating bathymetry is acceptable. More frequent updates in FS simulations involving dredging will need to be discussed.<br><br>Categories: HST Model<br>Sub-Categories: Bathymetry |
| 452 | Appendix M | Specific | Section 5.4.3, page 64, first paragraph, third sentence | Please correct the reference to "Attachment XXX" with the appropriate attachment. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |

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| 453 | Appendix M | Specific | Section 5.4.3, page 64, second paragraph, second sentence | Please correct the subject-verb agreement for "salt front…are" by either changing "are" to "is" or removing the parentheses around "and the ETM." | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 454 | Appendix M | Specific | Section 5.4.3, page 66, first paragraph, last sentence | Please correct the reference to "Appending [sic] XXX" with the appropriate attachment and revise "Appending" to "Attachment." | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 455 | Appendix M | Specific | Section 5.5, page 73, third paragraph, first sentence | Please revise the text to clarify whether "both cohesive sediments" refers to clays and silts. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 456 | Appendix M | Specific | Section 6.0, page 78, second paragraph, first sentence | Please revise the text to explain why the under-prediction of infilling and preferential infilling of the shoals is attributed to the sigma-coordinate system as opposed to other factors, such as critical shear stresses, as stated in the second numbered item in this sentence. | Part of the infilling may be explained by the bug identified by Region 2 in the CPG's implementation of the constant D50 option. The bug likely resulted in the calculation of lower shear stresses than expected in several cells in the shoals and may therefore explain preferential infilling of these areas. The text will be updated and/or enhanced as necessary following the final calibration simulations. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: RI Document, HST Model<br>Sub-Categories: Text Clarifications, Calibration |
| 457 | Appendix M | Specific | Figure 25 | The physical water column monitoring (PWCM) data collected prior to April 10, 2010, which the text indicates were excluded in the development of the rating curve (Section 4.2.4, page 42, first bullet, third and fourth sentences), should be identified on Figure 25 using different symbology. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 458 | Appendix M, Attachment B | General | | Throughout the attachment, the unit system of measurement is inconsistent. Measurements are sometimes reported in English units, sometimes in SI units, and sometimes in both. Please revise the attachment to ensure consistency in the system of measurement used. | The requested chage will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 459 | Appendix M, Attachment B | General | | Please revise the text to ensure that each figure is fully explained so that it is clear to the reader the point that is being made with each figure, and that figure legends identify all symbols and lines on the associated figure. For example, in Figure 2-3, the inset panels taken from Dr. Chant's work are not explained and would not be clear to many readers, and there should be a legend to clarify what the different colored lines indicate. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 460 | Appendix M, Attachment B | Specific | Section 2.0, pages 11 through 14 | The figure references within the text for Figures 2-2 through 2-6 include the figure number and title. Please revise the text to only include the figure number when referring to each figure. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |

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| 461 | Appendix M, Attachment B | Specific | Section 2.0, page 11, last paragraph, fifth and sixth sentences (continued on page 12), and page 12, Figure 2-3 | Please revise the text to provide a more detailed explanation of Figure 2-3, including the graphic overlays of Dr. Chant's data (with a reference to the report from which these overlays were taken). In addition, please revise the figure caption to be grammatically correct by adding the word "in" between "Computed variation" and "salinity intrusion," and add a legend to identify the blue and green lines. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 462 | Appendix M, Attachment B | Specific | Section 2.0, page 13, Figure 2-6 | Please revise the text to explain the figure in greater detail since there is a lot of information being conveyed (e.g., Little Falls and Dundee Dam flow; tidal range; likely scour events; etc.). Please include an explanation of the rectangle outlined in red on the left side of the figure. Please revise the note stating "no events between 2007 & 2008" to clarify that this is referring to the 2007 and 2008 multibeam surveys, since there was an erosion/scour event in early 2007. In addition, please update this figure since the 2011-2012 multibeam bathymetry survey data are available and can be included. Please remove the second part of the figure title ("from Fig. 2-1 it can be expected…"). Instead, since the 2011-2012 data can be evaluated, include a statement in the text that there were no major scour events that occurred between the 2011 and 2012 surveys. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications, Navigation Scour |
| 463 | Appendix M, Attachment B | Specific | Section 2.0, page 14, second paragraph, second sentence | Please revise the text to clarify how Figure 2-5 suggests that "elevated TSS values are to be expected at river flows beyond $Q_{riv}$ = 6000-7000 cfs." | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 464 | Appendix M, Attachment B | Specific | Section 2.0, page 14, second paragraph, third sentence | Please revise the text to clarify the statement that "Also from other observations, a similar threshold was found." The text should include specific mention of these "other observations" and where they are shown. | Will add similar plot with Chant's 0809 data. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 465 | Appendix M, Attachment B | Specific | Section 3.3., page 18, second paragraph, fourth sentence | Please revise the text to clarify what is meant by "significant" sedimentation by providing a quantitative measure. In addition, please note the typographical error and correct the spelling of "sedimentation" (currently written as "sedimention"). | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 466 | Appendix M, Attachment B | Specific | Section 4.2, page 22, Table 4-2 | Please change the criterion for mean bias in the performance testing over a soft bottom, with depths between 15 and 40 feet, to ± 0.2 feet since the mean bias may be positive or negative. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Bathymetry, Text Clarifications |
| 467 | Appendix M, Attachment B | Specific | Section 4.4, page 31, Figure 4-10 | Please add a legend to the figure to clarify what the horizontal lines represent. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures |

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| 468 | Appendix M, Attachment B | Specific | Section 4.4, page 31, Figure 4-10 | Please define the acronym LWR in the figure caption. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 469 | Appendix M, Attachment B | Specific | Section 4.5, page 33, Figure 4-11 | The figure legend is difficult to read. Please revise the figure to make the legend more legible and explain which colors denote erosion and deposition in the figure. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Bathymetry, Figures, Text Clarifications |
| 470 | Appendix M, Attachment B | Specific | Section 4.5, page 34, Figure 4-12 | Please revise the figure to correct the cropping of the right side of the legend labels. In addition, please clarify the meaning of the yellow coloring, which is labeled as "< threshold (.1 ft)," while the figure title states that "Bed level changes smaller than 0.5 ft are ignored as within accuracy." In addition, please move the latter portion of the figure caption, beginning with "Bed level changes…," to the description of Figure 4-12 in the text. Finally, please change the colors of the lines indicating RMs to be different from the colors used in the legend, since these lines have nothing to do with the erosion/deposition patterns at the associated RM. | The requested change will be made. | Response accepted<br><br>Categories: RI Document<br>Sub-Categories: Bathymetry, Figures, Text Clarifications |
| 471 | Appendix M, Attachment B | Specific | Section 4.5, pages 40 through 41, Figures 4-17 and 4-18 | Figures 4-17 and 4-18 are not referenced or described within the text. Please revise the text to ensure that all figures are discussed. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Bathymetry, Figures, Text Clarifications |
| 472 | Appendix M, Attachment B | Specific | Section 4.5, page 42, first sentence (continued from page 41) | Please revise the text to clarify the statement that "apparently, the flux towards the turbidity maximum is more important than the volume of sediment in the turbidity maximum itself." | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 473 | Appendix M, Attachment B | Specific | Section 4.5, page 44, last bullet | This bullet point seems out of place as the sentence above the bullet points implies that the discussion is regarding the source of fine sediments that fill scour holes, while this bullet point describes coarse sediments. Please revise the text to clarify how this point relates to the discussion of fine sediments. In addition, please correct the misspelling of "coarser" as "courser." | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Bathymetry, Figures, Text Clarifications |
| 474 | Appendix M, Attachment B | Specific | Section 4.6, page 44, second paragraph, first sentence | USACE (2010) provides the approximate drafts of vessels that transit certain areas of the LPR. Table 5 in the USACE report, which is based on a 1997-2006 study (nearly 10 years old), indicates that drafts up to 33 and 34 feet are observed, but these drafts represent less than 1% and 5%, respectively, of all trips recorded for the corresponding berths. In addition, these vessels remained near the mouth of the river, traveling no farther than RM 0.6.<br><br>It is also unclear how these deeper draft vessels would be able to transit any portion of the LPR at the present time, as a review of the 2012 bathymetry indicates that -26 feet National Geodetic Vertical Datum | The vessel drafts of 33 and 34 feet are based on historical navigation traffic studies, between 1997 and 2006. It is unclear what the water depths were at the time that such relatively deep draft vessels were navigating the lower 0.6 mile of the LPR. Therefore, these drafts (which also only represent 1% to 5% of reported drafts in 1997 to 2006) should not be compared to the 2012 bathymetry. It is also worth noting that the remaining 95% to 99% of vessel drafts only range up to 26 feet, which is within the range of current navigable depths in this portion of the LPR. | Based on discussions on the topic of navigation scour between the CPG and EPA modeling teams and presentations given by the CPG modeling team, the CPG's response to this comment is accepted, pending review of the revised sediment transport model code, inputs and results.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications, Navigation Scour |

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| | | | | (NGVD) is the (approximate) deepest bed elevation in the navigation channel, which is equivalent to -24.5 mean low water (MLW) in that area. Vessels with drafts deeper than this would require a very high tide in order to transit the river without impacting the sediment bed directly. Therefore, their frequency of transit up the river is likely to be relatively low. Based on these observations, it is reasonable to assume that, while vessels may have some impact on keeping the navigation channel in a sort of "depth equilibrium," the channel might also remain in equilibrium mostly as a function of hydrodynamics. Please revise the text to provide a more balanced discussion of the potential causes of this observed equilibrium in the channel. | Navigation scour is a complex phenomenon, generated due to the bed shear stresses induced by the turbulent flow field behind a rotating propeller. The rotating propeller produces a high velocity jet, the magnitude of which decays with distance radially from the propeller. As such, the bed shear stress is dependent not just on the rated draft of the vessel, but also the actual or loaded draft, height of the propeller shaft above the bed, propeller speed, whether the given vessel is maneuvering into/out of berths, and in the case of tugboats, whether the tugboat is towing a barge/vessel (possibility of scour), or in motion by itself (low possibility of scour). Calculations of the velocity profile induced due to propwash indicate significantly higher velocities and therefore shear stresses than tidal currents during low-flow/normal tidal conditions. Furthermore, shear stresses generated by tidal currents during low-flow/normal tidal conditions are too low compared to the critical shear stress defined for the surficial parent layer in the model, which suggests that local hydrodynamics cannot possibly explain the observed scour. Furthermore, these areas exhibit deposition during high-flow conditions in the LPR and erosion during low-flow conditions. It is highly improbable that areas which exhibit deposition during periods of above-average shear stresses (the high-flow conditions) will exhibit erosion during periods of relatively lower shear stresses (low-flow conditions) if the local hydrodynamics were the sole cause of the observed erosion/deposition pattern. This was the logic behind the hypothesis that vessel movement is the likely mechanism to explain the scour and the tendency to equilibrium depths noted in the various bathymetry surveys. | |
| 475 | Appendix M, Attachment B | Specific | Section 4.6, page 44, second paragraph, third sentence, and page 45, Figure 4-21 | Please revise the text to describe the procedure used to compute the water column velocities resulting from tug boat propellers, including the equation(s) used and appropriate references. In addition, please clarify whether the "distance from the propeller" referenced in the caption of Figure 4-21 is computed as vertical distance (i.e., depth below propeller), horizontal distance, or slant distance from the propeller. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications, Navigation Scour |
| 476 | Appendix M, Attachment B | Specific | Section 4.6, page 45, second sentence, and page 46, Figure 4-22 | Automatic Identification System (AIS) data should be retrieved for the same time period as the bathymetry survey data (i.e. 2011-2012) in order to provide a more representative comparison than the March-June 2013 AIS data. Please revise the text and figure accordingly once these data have been retrieved. | The CPG disagrees with Region 2's comment. The analysis was done for a random 1-week period, which will show the same features as if this is extended to a multiple week period of any year. This is standard practice in traffic analysis for waterfront/maritime infrastructure. There is no reason to believe that traffic in 2011 to 2012 was different from any other period. Data is available and the analysis could be done | Based on discussions on the topic of navigation scour between the CPG and EPA modeling teams and presentations given by the CPG modeling team, the CPG's response to this comment is accepted pending review of the sediment transport model code, inputs and results. |

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| | | | | | if unavoidable. However, this is not a trivial exercise, mainly due to the volume of data involved (records every few minutes for an entire year from hundreds of vessels potentially within the study area at any given moment). The result will, in all likelihood, be similar to the analysis already conducted. | Categories: RI Document<br>Sub-Categories: Text Clarifications, Navigation Scour |
| 477 | Appendix M, Attachment B | Specific | Section 4.6, page 45, fifth sentence, and page 46, Figure 4-22 | It is not clear from the figure that "the ship track and frequency maps show good correspondence with the areas of observed scour." The bathymetric difference panel illustrates erosion over nearly the entire area of study, so is it difficult to discern where the increased ship traffic density has caused specific patterns of erosion or scour. The ships are required to remain within the navigation channel to navigate safely, and the bathymetric change in the channel indicates erosion over most of the area of study, but, spatially, that appears to be the extent of the correlation in the data. Please revise the text to clarify how the data presented in the figure demonstrate correlation between ship traffic and observed scour. | The CPG will generate better graphics to illustrate the correspondence between ship tracks and observed patterns of erosion. | Response accepted, pending review of the revised text and figures.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications, Navigation Scour |
| 478 | Appendix M, Attachment B | Specific | Section 4.6, page 46, Figure 4-22 | Please revise the bathymetric difference panel to clarify that blue coloring indicates erosion and red coloring indicates deposition. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Bathymetry, Figures, Text Clarifications |
| 479 | Appendix M, Attachment B | Specific | Section 4.6, page 46, Figure 4-22 | The vessel density data show where the more frequent ship tracks were during this time period, but there is no information provided about vessel speed or draft, which would have an even greater impact on the resuspension of sediments. A risk rating system should be created that is a function of ship draft, speed, and track, which would better illustrate the spatial risk of resuspension. Please revise the text and figure to reflect this analysis. | As described previously (Comment 474), navigation scour is a complex phenomenon, dependent on various parameters, some of which are reported in the AIS records but others not. Although draft (this will be the rated draft not the actual loaded draft) and speed can be obtained from AIS data, the others cannot. The proposed analysis will also result in non-unique solutions since the number of unknowns (scour = function of speed, draft, propeller distance above bed, vessel motion type [i.e., maneuvering or sailing, etc.]) is larger than the number of knowns (2 values for erosion at a given location). Even if said analysis is done, it is unclear what would be obtained beyond what is already understood from these data, i.e., areas exhibiting erosion in the bathymetric surveys bounding low-flow periods are the ones at risk for navigation scour. | Based on discussions on the topic of navigation scour between the CPG and EPA modeling teams and presentations given by the CPG modeling team, the CPG's response to this comment is accepted pending review of the sediment transport model code, inputs and results.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications, Navigation Scour |
| 480 | Appendix M, Attachment B | Specific | Section 4.6, page 46, second paragraph, and page 47, Figure 4-23 | The text and figure imply that there is a large mass of eroded sediment that is available to the water column between RM 0 and RM 1.4 during low-flow periods in the LPR. However, it is unclear how much of this sediment is estimated to be transported upstream of RM 1.4 and how much is expected to be transported downstream to the Newark Bay Study Area (NBSA). Please revise the text and figure to clarify. | The CPG cannot answer this based on the available data. The exact fate of the eroded sediments is unknown. It depends on the timing of erosion (during flood or ebb tide). Furthermore, even if a sediment particle is eroded due to navigation scour during ebb, and therefore makes its way to NBSA, during the next flood tide that particle could make its way into the LPR, or the Hackensack, or if deposited in an ebb-dominant | Based on discussions on the topic of navigation scour between the CPG and EPA modeling teams and presentations given by the CPG modeling team, the CPG's response to this comment is accepted pending review of the sediment transport model code, inputs and results.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications, Navigation Scour |

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| | | | | | portion of NBSA, could be available for transport further afield in NBSA or even exported from NBSA. | |
| 481 | Appendix M, Attachment B | Specific | Section 4.6, page 49, third sentence | Not all of the vessel-scoured sediment in the lower miles of the LPR will be transported upstream to the LPRSA. Please revise the text to clarify how the resuspended sediment is thought to be distributed in terms of upstream vs. downstream transport. | The CPG cannot answer this question based on the available data. The exact fate of the eroded sediments is unknown. It depends on the timing of erosion (during flood or ebb tide). The text/figure was only meant to show that vessel scour likely produces a significant source of solids to the water column in the lower 1.4 miles of the LPRSA. The exact fate of these solids is unknown. | Based on discussions on the topic of navigation scour between the CPG and EPA modeling teams and presentations given by the CPG modeling team, the CPG's response to this comment is accepted pending review of the sediment transport model code, inputs and results.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications, Navigation Scour |
| 482 | Appendix M, Attachment B | Specific | Section 4.6, page 49, second sentence | Equilibrium bathymetry is also likely a strong function of the hydrodynamics of the LPR at a given point in time. While vessel traffic and types of vessels will have a stronger short-term impact on resuspended sediment, the hydrodynamics and tidal fluctuations of the LPR are likely to have a stronger long-term, continuous impact. Please revise the text to consider the relative impacts of short-term vs. long-term dynamics when evaluating sediment bed dynamics. | This appendix pertains only to data analysis. It presents navigation scour as the hypothesis to explain the observed erosion in the lower miles of the LPRSA. An analysis of short-term versus long-term impacts on sediment dynamics is beyond the scope of this appendix. | Based on discussions on the topic of navigation scour between the CPG and EPA modeling teams and presentations given by the CPG modeling team, the CPG's response to this comment is accepted pending review of the sediment transport model code, inputs and results.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications, Navigation Scour |
| 483 | Appendix M, Attachment B | Specific | Section 5.3, page 53, fourth sentence | It is true that the lowest contaminant concentrations were observed in areas with little to no sedimentation. However, some of the highest concentrations were also observed in such areas (e.g., Figure 5-4, 2008-2009 Data; Figure 5-5, 1995 Data; Figure 5-6, 2008-2009 Data). Please revise the text to clarify that the contaminant concentrations in areas with low sedimentation rates span up to four orders of magnitude in range. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 484 | Appendix M, Attachment B | Specific | Section 5.4, page 55, Figure 5-7 | Please revise this figure by adding the 1:1 line to allow easier visualization of how the cesium-based and bathymetry-based sedimentation rates compare. | The requested change will be made. | Response accepted, pending review of the revised figures.<br><br>Categories: RI Document<br>Sub-Categories: Figures |
| 485 | Appendix M, Attachment B | Specific | Appendix B, page 61, first paragraph, second sentence | Please revise the text to clarify when and why the 1-foot by 1-foot or 5-foot by 5-foot resolution data were used when evaluating the correction needed for the 2008 bathymetry. It is unclear why both resolutions are referenced within this appendix. Please make this clarification throughout Appendix B as needed. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Bathymetry, Figures, Text Clarifications |

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| 486 | Appendix M, Attachment C | General | | Although the entrainment rate analysis may provide a useful screening-level constraint on the shear stress-dependent erosion rate of the fluff layer, the limitations of the analysis should be presented more thoroughly in the report. In particular, the analysis is based on a very simplified approach to ST in the reach of RM 1.4 to RM 4.2 (Figure 2). Although the report makes an initial point of distinction between direct measurement of erosion rate (e.g., via SEDflume) and analysis of "entrainment rate" from differences in suspended sediment concentration (SSC) across the RM 1.4 to RM 4.2 reach, the ultimate application of the entrainment rate analysis is an erosion rate formulation for the fluff layer (Equation 2) that functions exactly the same as formulations of erosion rate from SEDflume measurements. However, unlike the measured shear stress in a SEDflume, the shear stress used in Equation 2 in this analysis is a spatial average of modeled shear stress for grid cells along the thalweg within the RM 1.4 to RM 4.2 reach. Even if one presumes that the model accurately simulates bed shear stress over the entire reach, the spatial averaging suggests that Equation 2 would tend to underestimate the fluff layer erosion rate for higher-than-average shear stress and overestimate it for lower-than-average shear stress, which could throw off the spatiotemporal response of the fluff layer dynamics.<br><br>As a "validation" step, it would be instructive to perform the entrainment rate analysis over a different time interval to assess the magnitude of variation in the results. Suitable supporting data are likely limited, so this validation might also be performed for arbitrary subsets of the existing time intervals. Please revise the text to discuss the results once this validation has been performed. | The analytical method used in the entrainment rate analysis has been updated. The average shear stresses between RM 1.4 and RM 4.2 has been replaced by a Lagrangian shear stress, in other words, the shear stress experienced by the fluid parcel during the time interval over which given entrainment rate is calculated. The resulting shear stress is a better estimate of the shear stress responsible for the measured entrainment rather than the average shear stress between RM 1.4 and RM 4.2.<br><br>The validation of the erosion properties resulting from this analysis is in its comparison to the Gust Microcosm data in Attachment D of Appendix M. However, the CPG will perform the requested sensitivity on the data analysis. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: HST Model<br>Sub-Categories: Erosion |
| 487 | Appendix M, Attachment C | Specific | Section C.1, page 2, first paragraph, third sentence | Please clarify the meaning of this sentence by deleting the extraneous word "and" from the phrase "the fluff layer and overlying less erodible strata." | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 488 | Appendix M, Attachment C | Specific | Section C.2, page 4, first paragraph, second complete sentence | Please revise the end of the sentence to read "RM 1.4-4.2." | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 489 | Appendix M, Attachment C | Specific | Section C.5, page 7, first paragraph, fifth sentence | Please revise the text to provide a quantitative measure of the proportion of the dataset affected by the variability in SSCs in lieu of the phrase "a relatively small subset of the entire dataset mainly at the lower velocities." Based on Figure 3, it appears that negative entrainment rates represent between approximately 15% and 20% of the dataset. Near-bottom velocities for these negative entrainment rates vary from approximately 0.3 to 0.7 meters per second (m/s), which encompasses a similar range to that | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |

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| | | | | of the positive entrainment rates, rather than being limited to the lower velocities. | | |
| 490 | Appendix M, Attachment C | Specific | Section C.6, page 8, Figure 4 | Please revise the figure to clarify if the black symbols represent the binned shear stress-entrainment rate pairs and whether negative entrainment rates were omitted before binning. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 491 | Appendix M, Attachment D | General | | This attachment compares the parameterization of fluff layer erosion developed using PWCM data to independent erodibility data collected by Chesapeake Biogeochemical Associates (CBA, 2006) using Gust microcosms. In general, the comparison is reasonable, given the uncertainties associated with both techniques. The critical stress chosen for the fluff layer parameterization, 0.25 dynes per square centimeter (dynes/cm2), is about 40% lower than the most frequent estimate of surface critical stress from the microcosm tests, 0.4 dynes/cm2, and the fluff layer critical stress remains constant while the microcosm critical stresses increase rapidly with depth. However, the reasonable agreement between the fluff layer erosion rate constant (both linear and power law forms; Figure 6) and those measured with the microcosm is encouraging. Similar comparisons in the literature often don't agree as well as those presented here. Ultimately, the chosen parameterization results in a greatly simplified but reasonable approximation of the PWCM data, in the same range as the microcosm results.<br><br>However, the presentation in this attachment overstates the similarities between the fluff layer parameterization and the microcosm data, particularly with respect to the critical stress profile. Furthermore, the derived fluff layer parameterization (Equation 3 and Figure 5) is not unique and should not be presented as such, as alternate combinations of parameter values can provide essentially the same outcome. | It is unrealistic to expect precise correspondence in absolute terms between parameter estimates derived from controlled laboratory measurements and from the use of a large-scale hydrodynamic model. Besides the deviations arising from any mismatch between actual and modeled currents, model results of skin friction in particular are also sensitive to other factors such as the parameterization of the Nikuradse grain roughness height used to compute skin friction. In ECOM_SEDZLJS, this is calculated as 2*D50; Soulsby (1997) gives an overview of this term, with alternatives ranging from 1.25*D35 to 3*D90. Thus, there is substantial disagreement about the value of this term, which in turn implies uncertainty in the skin friction computed by the model. Furthermore, even different measurement methods do not result in the same parameter values (see Tolhurst et al. 2000), which implies uncertainty in the data. This uncertainty needs to be kept in mind when comparing absolute values between the model and data. Nonetheless, comparison of the relative trends seen in the data (the 10X increase in critical shear stress within the top couple of mm of the bed) and that used in the model are still valid, and that was the point about comparing the critical shear stress profile between model and data. | The text of Attachment D needs to be revised to present a more-balanced discussion of the similarities between the fluff layer parameterization and the microcosm data, particularly with respect to the critical stress profile. The potential for alternate combinations of parameter values to achieve similar results should be discussed to address the issue of the uniqueness of the selected combination.<br><br>Categories: HST Model<br>Sub-Categories: Fluff Layer, Erosion |
| 492 | Appendix M, Attachment D | Specific | Section D.3, page 4, Figure 3 | Presentation of the microcosm critical stress profile data on log-log plots artificially accentuates the very near-surface and very low critical stress regions of the curves, making it appear as though the profiles exhibit an extended region of constancy near the surface, followed by a rapid increase to a high critical stress. In fact, the observed rate of critical stress increase with depth begins immediately and varies between a 0.5 to 1 power dependence on depth. In addition, the assumed constant critical stress of the fluff layer parameterization is not shown. Please revise this figure to present the data on linear axes and clearly show the chosen constant value of 0.25 dynes/$cm^2$ on all plots for comparison. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document, HST Model<br>Sub-Categories: Text Clarifications, Erosion |

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| 493 | Appendix M, Attachment D | Specific | Section D.4, page 6, Figure 5 | The agreement between PWCM-derived entrainment (erosion) rate and the parameterization of Equation 3 with $A = 1.1\text{x}10^{-5}$ cm/s/(dynes/cm$^2$)$^n$, $\tau_c$ = 0.25 dynes/cm$^2$, and $n$ = 0.75 is good. However, though the use of a linear (A=1x10$^{-5}$, $\tau_c$=0.25 dynes/cm$^2$, and $n$=1) fit is discussed in the text below the figure, it is not presented in Figure 5 for comparison. The linear fit is the basis of the comparisons to microcosm data in Figure 6 and therefore should be presented in Figure 5. While the chosen fluff layer erosion parameterization results in reasonable behavior, it is not necessarily the best possible or the only acceptable parameterization. Please revise the figure to include the linear fit and the corresponding $R^2$ value. The agreement of the linear fit with the data might be nearly as good as the nonlinear approximation, perhaps even better agreement if the first bin-averaged entrainment rate from the PWCM data is ignored. The main point is that, though the chosen fluff layer erosion parameterization results in reasonable behavior, it is not necessarily the best possible or the only acceptable parameterization. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document, HST Model<br>Sub-Categories: Text Clarifications, Erosion |
| 494 | Appendix M, Attachment E | General | | Please clarify the text throughout this attachment to ensure that the parameter averaging techniques are described consistently. To fully understand the procedures used to compute the sediment erosion parameters (e.g. *A*, *n*, and *EI*), detailed explanation of when arithmetic and log-averaging techniques were applied should be included. Refer to **Comment No. 506** for a specific example. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 495 | Appendix M, Attachment E | General | | Please clarify the text throughout this attachment to describe any filtering criteria that were applied to parameters when computing the critical shear stresses. For example, as discussed in **Comment No. 499**, please identify any filtering criteria that were applied to the regression coefficients, *A* and *n*, prior to averaging. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document, HST Model<br>Sub-Categories: Text Clarifications, Erosion |
| 496 | Appendix M, Attachment E | Specific | Section E.2, page 2, first paragraph, first sentence | Please revise the text to change the distance units between duplicate cores from feet to meters, consistent with units reported in Borrowman et al. (2006). As stated in that report, "Replicate cores were therefore anywhere from one to 10 meters apart from one another, which both explains some of the differences in bulk property and erosion rate behavior and illustrates the extent of heterogeneity of Passaic River sediments." | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 497 | Appendix M, Attachment E | Specific | Section E.2, page 2, first paragraph, fourth sentence | Please revise the text to clarify that 6 cores were collected from 3 core *locations* up-estuary of RM 8, and 22 cores were collected from 11 core *locations* between RM 0 and RM 8. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 498 | Appendix M, Attachment E | Specific | Section E.3, page 3, first paragraph, fourth sentence, and pages 10 through 37, | Please revise the text and figures so that the variable *d* represents the top of each depth interval, rather than the midpoint, to indicate the depth range over which each regression is applied. The actual length of each depth interval is unclear when using the midpoint of the depth interval in the figures. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |

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| | | | Figures 3 through 30 | | | |
| 499 | Appendix M, Attachment E | Specific | Section E.3, page 3, and Figures 3 through 30 | Please revise the text in Section E.3 to clarify how the *A* and *n* values were filtered when averaging and whether outlier *A* and *n* values were used in the averaging (e.g., Figure 3, subplot 6, lower right with n = 0.20413). In addition, please clarify whether $\tau_{Cr}$ values of zero (e.g., Figure 4, subplot 5, lower middle) or unusually large $\tau_{Cr}$ values (e.g., Figure 28, subplot 3, upper right with $\tau_{Cr}$ = 0.34 Pascal [Pa]) were used in the averaging. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 500 | Appendix M, Attachment E | Specific | Section E.4, page 4, text following Equation 3 | Please revise the text "as shown in Equations 3 and 4" to refer to Equations 4 and 5 instead. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 501 | Appendix M, Attachment E | Specific | Section E.4.1, page 4, first paragraph | Please revise the text to clarify whether the term "depth interval" is synonymous with "shear stress sequence." If not, please explain the difference between the two terms. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 502 | Appendix M, Attachment E | Specific | Section E.4.1, page 4, first paragraph, fifth sentence, and page 38, Figure 31 | Results for depth intervals containing only two data point pairs should not be included in the regression analyses or subsequent averaging because the uncertainty associated with the accuracy of the regression in determining the critical shear stress cannot be quantified without at least three data point pairs. Please revise the text and figure as necessary after removing these data from the analysis. | Erosion parameters from shear-stress sequences with only two data pairs were not included in the analysis to calculate erosion properties for the average core used in the model. This will be pointed out in the text. | Response accepted, pending review of the revised text.<br>Categories: RI Document, HST Model<br>Sub-Categories: Text Clarifications, Erosion |
| 503 | Appendix M, Attachment E | Specific | Section E.4.1, page 4, first paragraph, sixth sentence | Please revise the text to clarify whether the phrase "highly variable *EI* values" refers to comparisons of the *EI* values for the intervals with only two data pairs to *EI* values for the intervals above and below those intervals. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 504 | Appendix M, Attachment E | Specific | Section E.4.1, page 4, first paragraph, eighth sentence | Please revise the text to include discussion of those cores that are exceptions to the general trend described in this sentence (e.g., P09B, P07B, P03A, etc.). | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 505 | Appendix M, Attachment E | Specific | Section E.4.2, page 5, third sentence | The down-core trend of critical shear stress appears to be variable for many of the cores shown in Figures 32 through 35. Please revise the text to clarify and provide supporting analysis for the general statement that "the trend on average is of increasing critical shear stress with depth into the bed." | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 506 | Appendix M, Attachment E | Specific | Section E.4.3, page 5 | Please revise the text to clarify the averaging techniques that were applied at each step to the *A* and *n* values when computing the *EI* spatial patterns. For example, please clarify whether the log-averaged *A* and arithmetically averaged *n* values from each core were also log-averaged and arithmetically averaged, respectively, across the domain to compute a site-wide average. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |

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| 507 | Appendix M, Attachment E | Specific | Section E.4.3, page 5, last sentence | This sentence mentions a "lack of spatial variability in erodibility," which is inconsistent with preceding descriptions. For example, the sixth sentence in this paragraph notes that "*EI* is seen to vary over 2 orders of magnitude, with even the duplicate cores collected at a given location varying by an order of magnitude in some instances." Please revise the text to reconcile this and similar statements with the last sentence in the paragraph. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 508 | Appendix M, Attachment E | Specific | Section E.6, page 6, first paragraph, fifth sentence | Please revise the text to clarify how the erosion properties were averaged to develop a single set of parameters. For example, please clarify whether log-averaging or arithmetic averaging was used and whether erosion rates and power law regression coefficients were averaged.<br><br>Based on the language from the last sentence in Section E.1 ("The following sections describe….the resulting development of appropriate model inputs"), it is assumed that the "average core" described in Section E.6 is what is used as (or to develop) model input critical shear stresses. Please revise the text to clarify whether this is the case. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 509 | Appendix M, Attachment E | Specific | Section E.6, page 7, Table 1 | The values in Table 1 for the down-core critical shear stresses are lower than those in the CPG's model input files. Based on the ST model input file "erate.sdf," the down-core critical shear stresses are approximately 50% larger than the empirical critical shear stresses listed in Table 1. Please add a footnote to the table or revise the text to clarify whether the calculated or empirical critical shear stresses listed in Table 1 were utilized in the ST model and revise the text in Section E.6 to clarify how the model input files were generated. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 510 | Appendix M, Attachment F | General | | This attachment presents an analysis of water column monitoring data to derive characteristic bulk settling velocities for ST modeling, based on fitting classic Rouse SSC profiles to the observations. The data analysis and, as a result, the derived bulk settling velocities are reasonable, with a few minor caveats as discussed in the specific comments below. However, explanations of the observed behavior are biased toward the ultimate parameterization, while alternate explanations are discussed but ultimately ignored. More importantly, the ultimate parameterization for settling velocity (two particle classes with different settling velocities based on particle source) is presented very suddenly on page 8, in the last paragraph of the document, with very little connection to the preceding discussion. Furthermore, this parameterization is incompletely described and is not compared to the data, as discussed in **Comment No. 516**. While it may be a reasonable representation of settling behavior for modeling purposes, the presentation in the text needs to be expanded. Please revise this attachment to provide a more balanced and complete discussion of the parameterization selected as well as alternate possibilities. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |

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| 511 | Appendix M, Attachment F | Specific | Section F.2, page 3, Equation 2 | Overall, Sections F.1 and F.2 are well written and convincing, and the method proposed to derive empirical settling velocities is reasonable. However, it is unclear whether the turbulent Prandtl-Schmidt number ($\sigma_T$) of 0.7 used in Equation 2 is also used for the turbulent diffusivity of suspended sediment in the ultimate numerical model. Using $\sigma_T$ = 0.7 in the data analysis and then implicitly setting $\sigma_T$ = 1 in the numerical model (by ignoring it) would be equivalent to overestimating settling velocities by 1/0.7. Please revise the text to clarify whether the Prandtl-Schmidt number of 0.7 used in the analysis was carried through to the model. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 512 | Appendix M, Attachment F | Specific | Section F.3, page 4, paragraph after bullets, second sentence | Excluding data from the bottom-most and surface-most bins of the acoustic Doppler current profiler (ADCP) is reasonable, since they may be biased by blanking distance issues and spurious reflections, respectively. The explanation provided in the text, that they are excluded due to "suspected turbulence," should be expanded to specifically describe the type and source of the suspected turbulence. Please revise the text accordingly. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 513 | Appendix M, Attachment F | Specific | Section F.3, page 5, fourth paragraph | It is reasonable to dismiss flocculation as an explanation for the larger settling velocities during flood than ebb at RM 1.4. However, attributing this behavior only to mobilization of larger particles or transport of larger particles from downstream during flood is too restrictive. In fact, the second to last paragraph on this page implicitly points out the most likely reason for the observed behavior when it states that water column stratification causes the Rouse approach to fail (i.e., to overestimate settling velocities). It is apparent based on the salinity stratification during flood tide, shown in Figure 5, that the water column is frequently more stratified during flood at RM 1.4, while estimated settling velocities during flood are also larger. This is acknowledged in the discussion of Figures 12 through 17, but it is not acknowledged as a likely reason for the larger settling velocities observed during flood at RM 1.4. Please revise the text to include this explanation, even if it does not immediately support the final parameterization chosen. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 514 | Appendix M, Attachment F | Specific | Section F.4, page 7, fourth paragraph | The paragraph discussing potential relationships between settling velocity and SSC is overly dismissive. While it is true that the data shown in Figures 21 through 23 indicate a significant amount of scatter, a general pattern of higher settling velocities at higher SSCs is apparent. Figure 23 in particular shows a clear relationship between settling velocity and SSC at higher SSCs at RM 13.5. More importantly, while this analysis shows clear relationships between near-bottom water velocity and bulk particle settling velocity, it is unclear if or how these relationships are used in the model. If higher settling velocities at higher flow velocities are associated with resuspension of larger particles, as stated in the text, there should be an associated correlation between SSC and settling velocity, as more total particles should also be resuspended at higher flow velocities. However, as currently described in this attachment, the settling velocity parameterization does not explicitly incorporate either of these factors. Please expand the | Spatial patterns of settling velocity as a function of SSC can be further elaborated upon. | The response is accepted pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |

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| | | | | discussion in the text to more completely address the relationships among near-bottom velocity, mass and size of resuspended solids, and settling velocity. | | |
| 515 | Appendix M, Attachment F | Specific | Section F.4, page 7, last paragraph | Figure 25 is a good summary figure showing an upstream decrease in median settling velocity. However, the explanations for this trend presented at the bottom of page 7, while plausible, are not the only possible explanations and are not clearly related to most of the previous discussion. Please revise the text and provide additional figures to more explicitly demonstrate the connections between these potential explanations for the upstream decrease in settling velocity and the previously described behaviors. This might include, for example, a graph depicting an upstream decrease in tidal velocity that accompanies the observed upstream decrease in median settling velocity. | This will be clarified in the text. However, the decrease in tidal currents with distance up-estuary is the expected behavior for any tidal system since tidal prism decreases with distance up the estuary. This can be described in the text with reference to plots in Appendix L. | The response is accepted pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 516 | Appendix M, Attachment F | Specific | Section F.4, page 8 | The parameterization of the observed behavior as the sum of the behaviors of two independent particle classes of different origin and different settling velocities is abruptly proposed at the end of Section F.4. While this may be a reasonable way to parameterize sediment settling behavior in the LPR, it is not necessarily the only possible explanation and its relationship to the preceding analysis needs to be more explicitly described. Please revise the text to clarify how this parameterization helps to explain the previous observations of relationships between water velocity and settling velocity at different locations; how these two particle classes will be implemented and what their characteristic settling velocities are; whether there will be any exchange between the two classes; and how this parameterization compares to the previously presented data. | This text pertains to model parameterization whereas this appendix is only about the data analysis. Will remove this text. | The text of Attachment F begins and ends with statements about how settling will be specified in the model. The purpose of this section is to support the parameterization of settling in the model. The requested changes should be made rather than suggesting that this is a stand alone data analysis with no connection to the model.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 517 | Appendix N | General | | The text in the introduction states that ST-SWEM was eliminated from the CPG modeling approach in part due to schedule concerns. The ST-SWEM carbon model simulation speed is as fast as or faster than the CFT model and considerably faster than the ST model. The carbon model could have been implemented without modification with little or no impact to the overall schedule. In addition, the CPG has stated that they have concerns with the ST-SWEM model calibration, and EPA agrees that modifying the calibration to address those concerns would likely impact the schedule. Given the CPG's concerns, the CPG should present the OC model (referred to as the Alternative Organic Carbon [AOC] approach) results compared to site data. Please revise this appendix to present these results as implemented in the RI/FS rather than modified to match ST-SWEM, for a long-term (1995-2013) simulation, on a spatial and temporal basis. The | The concern here is two-fold: 1) the way the AOC model was run (using time variable foc, i.e., mass conserving for bed POC) for the comparison with ST-SWEM is not how it was run for the RI/FS (temporally constant foc, i.e., not mass conservative for bed POC); and 2) the lack of model-data comparisons from a long-term simulation. As described during the June 28, 2016 modeling meeting (also in Comment 518), the CPG proposes to revert to the time-variable foc approach barring adverse impacts on CFT model performance, which will address the former concern. The latter concern will be addressed by doing the long-term model-data comparisons asked for (also in Comment 520). | The response is accepted pending review and approval of the revisions to the organic carbon model code, inputs, outputs and associated text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: OC Model<br>Sub-Categories: Mass Balance |

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| | | | | results presented here, which were modified to match ST-SWEM for a year, do not reflect how the model was applied to the RI/FS. | | |
| 518 | Appendix N | General | | The CPG's OC modeling approach does not conserve the mass of OC in the sediment. For the modeled contaminants (2,3,7,8-TCDD and tetra-PCB), nearly the entire contaminant mass in the sediment is particle-bound. Because contaminant resuspension is tied to the computed solids resuspension, virtually the same contaminant mass would resuspend regardless of the POC concentration. While this may not be important in terms of resuspension of contaminants, it may affect the partitioning in the water column and the flux of contaminants back to the sediment. If the CPG wishes to use their current approach, a sensitivity analysis should be performed to demonstrate that the lack of a POC mass balance around the sediment does not adversely affect CFT model contaminant predictions in the sediment and water column. Please revise this appendix to include discussion of the results once this analysis has been performed. | As discussed with Region 2 during the June 28, 2016 modeling meeting, the CPG agreed to implement the mass balance approach as part of the OC simplification agreement. The OC model predictions affect not only the CFT model but also the bioaccumulation model. It is critical for the OC model to reproduce observed bed carbon concentrations so that reasonable carbon-normalized values can be provided to the bioaccumulation model. If reasonable bed OC predictions cannot be achieved, the CPG will switch back to the constant foc approach, and document the findings as requested by Region 2 during the meeting. | The response is accepted pending review and approval of the revisions to the organic carbon model code, inputs, outputs and associated text. If the mass balance approach does not work, the reasons for the imbalance should be explored, identified and addressed. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: OC Model<br>Sub-Categories: Mass Balance |
| 519 | Appendix N | General | | The presentation of information in this appendix is not as clear as other portions of the RI Report. It reads more like an outline than a report in some sections (e.g., Section 2) and includes grammatical errors (e.g., "A subsequent sections will address the importance of this on the contaminant simulation results." on page 10). Please review the entire appendix and make corrections as necessary. In addition please replace references to "water phase" with "water column" to be consistent with the other sections of the RI. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 520 | Appendix N | General | | The period of 1 year used to compare the two approaches to modeling OC is too short. The difference between the bed concentrations in the two models at the end of 1 year is approximately 13%. As stated in the second paragraph on page 10, the AOC approach shows an approximately 6% increase in POC in the active layer at RM 2.0, while the ST-SWEM model shows an approximately 7% decrease. This occurs despite the ST-SWEM model having a higher concentration of algal carbon throughout most of the year, in particular June through September, when the ST-SWEM algal carbon concentrations are generally several times greater than the AOC concentrations (Figure 2). Part of the difference in bed POC might be explained by the fact that the AOC model uses a constant settling rate for phytoplankton, while ST-SWEM uses a temperature-dependent rate tied to the fact that the viscosity of water, which affects settling rate, is a function of temperature (ST-SWEM uses an Arrhenius temperature coefficient or θ | At this point it is not clear how either ST-SWEM or AOC will perform relative to the data in the long-term comparisons. Beyond the 1-year comparison of the AOC and ST-SWEM results already included in the report, should the aim of the AOC approach be to demonstrate similarity with ST-SWEM or similarity with the data? The CPG's goal is the latter; the model-data comparisons called for in Comment 517 should address this point. | The goal should be for the model to demonstrate similarity with the data, while also capturing the major transport processes controlling the fate of particulate organic carbon in the system.<br><br>The response is accepted pending review and approval of the revisions to the organic carbon model code, inputs, outputs and associated text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: OC Model<br>Sub-Categories: Mass Balance |

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| | | | | of 1.027). The CPG should consider implementing a temperature-dependent settling rate in the AOC approach as well. However, the primary concern is whether the POC concentrations in the two models continue to diverge over time and by how much, which should be demonstrated with decadal-scale simulations. Please revise this appendix to present the results once these simulations have been performed. | | |
| 521 | Appendix N | General | | The discussion in Section 4.2 concerns whether to assume time-variable or temporally constant OC dynamics in the sediment bed. The decision to simplify the OC model to eliminate the explicit computation of phytoplankton growth and the multiple detrital carbon classes in the sediment bed would still require the computation of time-variable OC dynamics in the sediment bed. Please revise the text to provide justification for the assumption of a constant $f_{OC}$ in the bed, along with results for a multi-year run performed using both approaches to evaluate the impact of a constant $f_{OC}$ on the CFT model results. | See Responses to Comments 517 and 518. Text will be revised pending on the final OC approach. | The response is accepted pending review and approval of the revisions to the organic carbon model code, inputs, outputs and associated text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: OC Model<br>Sub-Categories: Mass Balance |
| 522 | Appendix N | Specific | Section 1.0, page 5, third paragraph, first two sentences | Although the OC simplification proposed by the CPG was accepted in concept, there are some outstanding concerns (refer to **Comment Nos. 517 through 534**). These concerns must be addressed before the RI can state that there is a consensus on the CPG's approach. | See Responses to Comments 517 through 534. Text will be revised accordingly pending on the final OC approach. | The response is accepted pending review and approval of the revisions to the organic carbon model code, inputs, outputs and associated text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: OC Model<br>Sub-Categories: Mass Balance |
| 523 | Appendix N | Specific | Section 2.2, page 6, fourth bullet | Please remove this bullet from the text if the resuspended POC is based entirely on assigning a fixed $f_{OC}$ to the bed, as stated in Section 4.2. If the approach is modified such that the bed POC is computed as a mass balance assuming an 85% loss of newly deposited algal carbon and no loss of the conservative detrital carbon, no edit is needed. | See Response to Comment 518. Text will be revised pending on the final OC approach. | The response is accepted pending review and approval of the revisions to the organic carbon model code, inputs, outputs and associated text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: OC Model<br>Sub-Categories: Mass Balance |
| 524 | Appendix N | Specific | Section 2.2, page 7, second bullet, and Section 2.3, third bullet | In the test results presented in Section 3.0, a mass balance was computed for the bed POC. However, the mass balance approach is not used when the CPG applies the model in the RI/FS, and therefore the test is not directly applicable to the CPG's modeling approach. The initial approach for simplifying the OC model which the CPG described in conversations with EPA and their consultants would have used a fixed ratio to the total solids in the bed; however, as described in this document, the bed POC was set as a | See Response to Comment 518. Text will be revised with greater details, pending on the final OC approach. | The response is accepted pending review and approval of the revisions to the organic carbon model code, inputs, outputs and associated text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review. |

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| | | | | fixed ratio to the cohesive solids rather than the sum of the non-cohesive and cohesive solids. The description, testing, and application of the OC model should use the same approach to handling bed carbon. These bullets need to be expanded to clarify how the data were used to define the carbon concentration of the resuspended solids. | | Categories: OC Model<br>Sub-Categories: Mass Balance |
| 525 | Appendix N | Specific | Section 2.3, page 7, first bullet | Please revise this bullet to include the resuspension of sediment POC as a source of water column POC. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 526 | Appendix N | Specific | Section 3.0, pages 8 through 18 | Please replace or supplement Section 3.0 with a demonstration of the model's ability to reproduce carbon data, particularly water column POC, as well as a demonstration of the impact of the lack of a carbon mass balance. | See Responses to Comments 517 and 518. Requested model-data comparison will be provided for Section 3. | The response is accepted pending review and approval of the revisions to the organic carbon model code, inputs, outputs and associated text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: OC Model<br>Sub-Categories: Mass Balance |
| 527 | Appendix N | Specific | Section 3.2, page 9, first sentence | Please remove the reference to grid cell 323, as the reference is unclear and the RM and grid indices are sufficient to identify the location. Results for additional locations should also be presented with the CPG's OC model compared to data. | The requested change will be made. | Response accepted, pending review of the revised figures and associated description in the text.<br><br>Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |

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| 528 | Appendix N | Specific | 361 Section 4.1, page 19, second paragraph | The approach described in the text would maintain the mass and average bulk density for the two layers (the active layer equal to the top 15 cm and the archive layer equal to the remainder of the bed). The bulk density of both layers would therefore be overestimated at the surface and underestimated at the bottom. This error would be small for a single point in time, but may present issues due to the time variable nature of the bulk density computed by the ST model.<br><br>If the bed OC is computed based on a mass balance, the constant thickness of the active layer will result in continuous numerical mixing of POC between the two layers each time there is deposition and subsequent erosion. If the bed fOC is kept constant, this numerical mixing will not be an issue. The CPG will have to provide additional diagnostic information before EPA can approve the use of the CPG's current, constant fOC, approach (refer to Comment No. 521).<br><br>Based on the text it is not clear how the CPG has handled consolidation, a concern previously noted by EPA. For example, if a large amount of cohesive sediment is deposited following a storm, and consolidation then occurs without any additional deposition or erosion, both the active and archive layers will increase in bulk density and decrease in volume. Assuming 30 cm of deposition, 15 cm would initially be added to both the active and archive layers. Following consolidation without any additional erosion or deposition, the thicknesses would be 15 cm and 8.3 cm in the active and archive layers, respectively, using the CPG's ST model parameterization. It is unclear how the CPG's version of the OC model handles this bed elevation change. Given the fixed volume structure of the CFT model bed layers below the top layer, it is also unclear how this change in bulk density is handled when it is passed forward from the OC model to the CFT model. Finally, it is unclear what errors would result if the thickness change is applied at the sediment-water interface, rather than distributed over depth, and weighted toward the bottom of the deposit. The resulting volume change should not be applied only to the top layer or the very bottom layer in either the OC or CFT model. As previously stated, this is not a concern in the OC model if the ratio of POC to cohesive solids is constant and a mass balance is not achieved. The primary concern is in the CFT model, where the archive stack is not set up to handle changes in layer thickness over time.<br><br>Please revise the text to address how consolidation was handled in the revised code to ensure that the transfer of solids, OC and contaminants between the CFT model layers is represented consistently. If the contaminant mass in a layer is conserved, but the bulk density of that layer | There are a number of comments and corresponding responses:<br><br>1) The issue of bulk densities being over-estimated at the surface and under-estimated at the bottom is not unique to AOC but is a common feature shared with ST-SWEM and derives from the overall modeling framework for the LPRSA.<br><br>2) The issue of potential numerical mixing may turn out to be a non-issue since both ST-SWEM and AOC only have two bed layers, and so the model framework inherently averages out vertical gradients in POC such that numerical mixing could be a non-issue. The CPG will evaluate this in the calibration run with time-variable foc by examining temporal changes in archive layer foc/POC in cells without any appreciable bathymetric change over the long term. Large changes would be indicative of numerical mixing. If proven to be an issue, this artifact can be readily addressed as well.<br><br>3) Agree, the current text is missing information on how consolidation is handled. The bed thickness/composition for active/archive layers written by the sediment transport model is based on the fully consolidated bulk densities, so the sort of artifacts described in the scenario in the comments will not arise. Will add this description to the text.<br><br>Note that the bulk density representation in the OC and CFT models has been discussed with Region 2 at the June 28 and September 20, 2016 modeling meetings. The CPG concluded the variability of Layer 10 dry-weight contaminant concentrations noted by Region 2 was largely due to the bulk density gradients between the active and archive layer, and has little impact to the 15-cm average concentrations. It was agreed at a follow-up interaction with Region 2's modeling team (January 6, 2017) that no change to the existing bulk density approach is needed at this time, but Region 2 asked that CPG check the predicted contaminant concentrations in areas with large deposition once other model updates are done. | 1) Revisions that the CPG made to the model exacerbated the existing discontinuity in bulk density present in ST-SWEM. The CPG can use their approach but will need to look closely at results where there is a substantial amount of deposition or erosion, and in particular areas with substantial deposition followed by subsequent erosion. As noted by the CPG, the results for any such locations should be presented along with any impacts on predicted 15 cm surface sediment concentrations on a normalized basis.<br><br>2) The CPG will need to present its evaluation to support their conclusion that there is not an issue with numerical mixing between the active layer and the deeper sediments once they have completed their other changes to the HST and OC models.<br><br>3) The use of the fully consolidated bulk density profile does address the concern with the time history component of consolidation, but not the impact of the discontinuity between conserving mass and volume noted above in #1 (i.e. changing the bulk density without changing the volume results in a change in mass).<br><br>Categories: OC Model<br>Sub-Categories: Bulk Density |

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| | | | | is adjusted without changing the volume, the result is a change in dry weight concentration, which should not occur. | | |
| 529 | Appendix N | Specific | Section 4.2, page 20 | The chosen approach does not conserve the mass of POC in the sediment. Please revise this section to address the impact of the chosen approach, particularly the flux of carbon to the water column and the resulting impact on the flux of carbon to the sediment on the computed contaminant results. | See Response to Comment 518. Text will be revised pending on the final OC approach. | The response is accepted pending review and approval of the revisions to the organic carbon model code, inputs, outputs and associated text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: OC Model<br>Sub-Categories: Mass Balance |
| 530 | Appendix N | Specific | Section 4.3, page 20, first paragraph, second sentence | Please revise the text to clarify whether the three EPA carbon pools (inert, labile, and refractory carbon) have been appropriately summed into the single carbon pool represented in the OC simplification. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |

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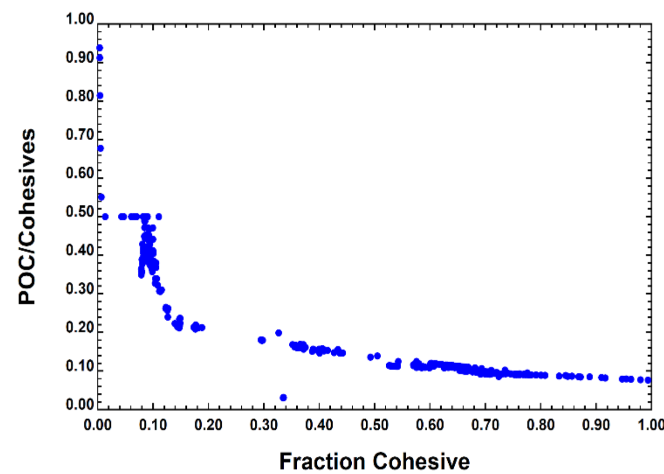
| 531 | Appendix N | Specific | Section 4.3, page 20, second paragraph, third sentence | A maximum value of 50% carbon on cohesive sediments is at the high end of the range for pure organic matter. In addition, application of the minimum 5,000 milligrams per liter (mg/L) POC results in a ratio of POC to cohesive solids as high as 94%. The figure below (created by HDR using CPG's model outputs) presents this ratio plotted against the sediment fraction cohesive using output from the CPG's CFT model for time zero of the long-term calibration for 2,3,7,8-TCDD for the grid cells within the study area. Please adjust the maximum value for carbon on cohesive sediments and the minimum value for sediment POC so that it does not result in $f_{OC}$ values greater than the stated maximum.<br><br> | Those high foc values were an artifact of having a minimum POC concentration in grid cells with limited cohesive solids (nearly zero fraction of cohesive). The approach will be revised to eliminate this artifact. | The response is accepted pending review and approval of the revisions to the $f_{OC}$, fraction cohesive relationship and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: OC Model<br>Sub-Categories: Bed $f_{OC}$ |
| 532 | Appendix N | Specific | Section 4.3, page 20, second paragraph, and page 21, Figure 16 | Based on Figure 16, the data suggest that a power function would fit the observed relationship between $f_{OC}$ and bulk density better than a linear regression. Please reevaluate the data presented in Figure 16 and discussed in Section 4.3 to determine whether an alternative analysis would provide a better representation of the relationship between $f_{oc}$ and bulk density. | See Response to Comment 397. | The response is accepted pending review and approval of the revisions to the $f_{OC}$, bulk density relationship and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: OC Model<br>Sub-Categories: Bed $f_{OC}$ |
| 533 | Appendix N | Specific | Section 4.3, page 22, Figure 18 | Please revise this figure after reevaluating the data presented in Figure 16 using a power function, as requested in **Comment No. 532**, in order to obtain a better comparison between the WY9596 initial condition and the observed data in the lower portion of the LPR (RM 0 to RM 7). | See Response to Comments 397. Figures 17 and 18 will be revised accordingly. | The response is accepted pending review and approval of the revisions to the $f_{OC}$, bulk density relationship and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: OC Model<br>Sub-Categories: Bed $f_{OC}$ |

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| 534 | Appendix N | Specific | Section 5.0, page 23, third paragraph, sixth sentence | Although the differences between the two approaches had only a minor effect on the model results, the model has not been implemented as tested, and therefore the actual differences in the results may be larger than presented in Appendix N. As noted in **Comment No. 520**, the testing should also be conducted for a longer period to verify model performance. Please revise this appendix to present OC model results compared to water column POC and chlorophyll-*a* data and ST-SWEM, using a longer-term simulation (1995-2013) and running the model as it was implemented in the RI/FS. Prior to completing this comparison, please address concerns over the lack of a carbon mass balance (**Comment Nos. 518, 523, 524, and 526**) and how consolidation will impact both OC and CFT model results (**Comment No. 528**). | See Response to Comment 517. Time-variable foc will be re-evaluated and adopted barring adverse impacts on CFT model performance; long-term model-data comparisons will also be performed. | If the mass balance approach does not work, the reasons for the imbalance should be explored, identified and addressed.<br><br>Categories: OC Model<br>Sub-Categories: Mass Balance |
| 535 | Appendix O | General | | Technical arguments can be made to support the modifications to the CFT model related to the fluff layer, vertical mixing parameterization, and non-equilibrium partitioning, but together these modifications tend to minimize the quantities of contaminants that are near the surface layer of the bed and that can be reintroduced into the water column. This appears to result in a more rapid "natural recovery" in strongly depositional areas, which is not supported by the data (see Figures 15a and 15b in Appendix K; in the bottom panel, labeled "Strongly Depositional," the concentration decreases in time and underpredicts the data in 2010 due to the rate of decline between 1995 and 2010). This discrepancy is also evident in comparing the solids-normalized model results to the data, as shown in Figures 4-3a and 4-3b (in the middle panel, labeled "Depositional," the distribution of model results falls well below the distribution of the data). The CPG hypothesizes, perhaps in an effort to correct this discrepancy that contaminants may be sorbing to non-cohesive solids. The justification for this hypothesis, discussed in **Comment No. 562**, is presented in part in Section 4.2, in the second paragraph on page 38, with reference to Figures 4-4a and 4-4b. However, it is not clear from these figures that the model is biased low in areas with a fine/cohesive sediment fraction less than 20%. For 2,3,7,8-TCDD, there are only two data points between RM 1 and RM 7 with which to make a comparison. Between RM 0 and RM 17, where there are more data, the model appears to bracket the two very low data points (less than 5% fine sediments and a concentration of approximately 1 ng/kg) and the three higher data points (between 5% and 15% fine sediments and concentrations between 10 and 20 ng/kg). Furthermore, the model appears to be biased low for both 2,3,7,8-TCDD and tetra-CB in areas with a fine/cohesive sediment fraction between 20% and approximately 50%. This may be due to the combined effects of the fluff layer, vertical mixing parameterization, and non-equilibrium partitioning, which would also affect areas with lower OC content. This bias must be corrected and the text updated to reflect the corrections before the model can be used for application to the RI/FS. | See Response to Comment 373. The CPG will consider the factors Region 2 raised in this comment and in Comment 562 when re-assessing the performance of the updated model. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: CFT Model<br>Sub-Categories: Partitioning, Fluff Layer, Sediment Mixing |

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| 536 | Appendix O | General | | The use of the term “parent bed” in this appendix is inconsistent with its use in the ST model. In the ST model, the parent bed refers to sediment in place at time zero of the simulation with any deposition on top going into depositional layers. In Appendix O, “parent bed” refers to any bedded sediment at the surface of the active layer. Please revise the text to use a different term for the surficial bedded sediments in the CFT model. Note that the comments on Appendix O use the term “parent bed” for consistency with this document. | The terminology will be adjusted and/or clarified to avoid confusion. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 537 | Appendix O | General | | Results of each of the model sensitivity analyses discussed in the RI Report should be presented in figures and tables for comparison to the chosen set of calibration parameters. It is not necessary to present every iteration of the model, but where it is stated that the result had a significant impact on the model calibration or resulted in significant deviations from the previous model application, the results should be presented in the RI Report (e.g., computed water column and sediment concentrations should be presented for the model before and after the addition of the fluff layer and partitioning changes). | Sensitivity analyses will be included in the revised RI Report. | The response is accepted pending review and approval of the revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: RI Document, CFT Model<br>Sub-Categories: Figures, Text Clarifications, Calibration |
| 538 | Appendix O | Specific | Section 2.1.1, page 3, bullets | The example presented in this section suggests that the solids in the surface layer (top 0.5 to 2 cm) of the sediment bed have a contaminant concentration 10 times higher than the water column solids. The water column contaminant data and sediment contaminant data from the top 0.5 feet of the bed generally do not support a gradient of that magnitude. The contaminant concentrations at the surface of the sediment are more likely to fall in between 100 and 1,000 ng/kg, resulting in a lower ratio. If the concentrations in the surface layer were as high as 1,000 ng/kg, the solids in the water column would likely reflect higher concentrations. Please revise this section to present model results for a longer-duration run (1995-2013) with and without the fluff layer incorporated to demonstrate how the fluff layer impacts the model results. | The example presented uses rounded hypothetical numbers to illustrate conceptually that the presence of a fluff layer would be expected to influence the contaminant exchange between the water column and sediments, and not representing the fluff layer may impart a bias in the model predictions. The extent of that bias is dependent on multiple other factors and will not be constant in time or space. Whether the actual concentration difference between the water column and sediment is tenfold or something lower is immaterial to demonstrating the conceptual point that the fluff layer was added to allow the CFT model to be able to represent gradients that may arise between the top cm or two of the sediment bed and the solids actively resuspending and depositing on each tidal cycle. The text will be clarified to emphasize the conceptual nature of the discussion and the use of hypothetical numbers.<br><br>The requested sensitivity runs will be conducted and included in the revised RI Report. | The issue with the example is that it suggests that such a large gradient could exist between the water column and top couple of centimeters of the sediment and if it did temporarily exist that it should be maintained.<br><br>Response accepted, pending review of the sensitivity runs and revised text.<br><br>Categories: CFT Model<br>Sub-Categories: Fluff Layer |

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| 539 | Appendix O | Specific | Section 2.1.1.1, page 4, fifth and sixth bullets and footnote 1 | This combination of assumptions will eliminate diffusive exchange between the parent bed and the water column when there is a fluff layer present. Because the fluff layer thickness transfer to the parent bed is an exponential decay term, the only time the fluff layer will not be present is under continuously erosional conditions. Furthermore, the model code includes a minimum fluff layer thickness (presently set as 1 µm, or half the thickness of one clay particle), ensuring that there is always a fluff layer present even under continuously erosional conditions. It is not valid to assume that tidal resuspension is much greater than diffusive exchange at all times and in all locations. Although diffusive exchange is not a significant factor for the contaminants tested in the reach-scale mass balance presented in RI Report Figure 7-3, it may be important for other COPCs that must be addressed by the RI/FS, or on a more localized basis. Diffusive exchange between the adjacent layers of water column, fluff layer, and parent bed or water column and parent bed must be represented in the model. Please revise this section accordingly once the necessary changes to the model have been made. | As discussed with Region 2 during the June 28 modeling meeting, the CPG will include the diffusive exchange between the fluff layer and the water column, or between the surface bed layer and the water column when the fluff layer is not present.<br><br>With regard to Region 2's concern that "the model code includes a minimum fluff layer thickness … ensuring that there is always a fluff layer present even under continuously erosional conditions," it is noted that the fluff layer is considered depleted when it reaches its minimum thickness and does not prevent exchange between the surface bed layer and the water column in this case. The small minimum thickness is maintained purely for numerical stability reasons.<br><br>With regard to Region 2's concern that "[b]ecause the fluff layer thickness transfer to the parent bed is an exponential decay term, the only time the fluff layer will not be present is under continuously erosional conditions," it is noted that the thickness transfer exists to decay away excess fluff thickness such that the fluff layer consists only of material "going up and down" (see notes provided to Region 2 on June 27, 2016). Once the excess thickness has been removed by the thickness transfer, the fluff layer will be created and fully depleted on each tidal cycle; continuously erosional conditions are not necessarily required to expose the surface sediment layer to the water column, as Region 2 suggests. | The EPA team will review the revised fluff layer model code once it is provided by the CPG to verify that it is representing diffusive exchange with both the bed and water column when present, and between the sediment and water column when the fluff layer is not present.<br><br>Categories: CFT Model<br>Sub-Categories: Fluff Layer |
| 540 | Appendix O | Specific | Section 2.1.1.1, page 4, seventh bullet | The fluff layer cannot have the same bulk density as the surface layer of the parent bed since, as stated in the first paragraph in Section 2.1.1.2, the fluff layer is "a thin surface layer of *unconsolidated* sediment." Please make the necessary corrections to the model to represent the fluff layer using the properties computed by the ST model and revise the text accordingly. | As discussed at the December 16, 2016, the CPG proposed to maintain the present assumption of uniform bulk density across the fluff layer and underlying surface layer in representing the contaminant mass transfer between these layers; this exchange will be modeled on a total chemical basis with a calibrated mass transfer rate (per Responses to Comments 405 and 542) and the influence of bulk density gradients would be small and lumped into the calibrated value of the transfer rate. Note that the RCATOX mixing calculation between underlying sediment layers also does not account for gradients in bulk density or sediment composition. If fluff layer dry weight concentrations are needed for plotting, the CPG proposed to calculate them using a near-surface bulk density from the ST model (e.g., 1 mm) as an approximation. During a follow-up interaction on January 6, 2017, Region 2 indicated that these approaches were acceptable. | The EPA team will review the revised fluff layer model code once it is provided by the CPG to verify that it is representing diffusive exchange with both the bed and water column when present, and between the sediment and water column when the fluff layer is not present.<br><br>The CPG's upscaling concerns should be addressed by the approaches discussed during follow up meetings. Once they have tested their proposed approach in the model and provided the revised model it will be reviewed and any necessary comments provided.<br><br>Categories: CFT Model<br>Sub-Categories: Fluff Layer |

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| | | | | | Also discussed at the December 16, 2016 meeting was the related topic of CPG's proposal to address erosion "upscaling" concerns by using a more representative cohesive concentration to compute the erosion velocity term. This term is the primary influence of bulk density (sediment composition) on the flux of contaminant to the water column, and the improved representation will implicitly account for the unconsolidated nature of the fluff layer relative to layers below. Region 2 agreed with the proposed solution conceptually, and additional interactions with Region 2 on implementation details are anticipated.<br><br>The revised RI Report will contain a description of the fluff layer bulk density treatment outlined above, augmented with details on the finalized implementation. | |
| 541 | Appendix O | Specific | Section 2.1.1.2, page 5, Equations 1 and 2 | Although the CPG has shut off partitioning to DOC in the water column, the model appropriately represents partitioning to DOC in the sediment bed. If the partitioning behavior in the fluff layer matches the parent bed, then these two equations should include DOC partitioning. Please correct these equations and the subsequent equations that depend on Equations 1 and 2 (Equations 3 through 17). | The equations and accompanying text will be adjusted per Response to Comment 542, but will not include DOC-bound contaminant explicitly because the exchange will be modeled on a total concentration basis. The reasons for simulating the exchange on a total concentration basis were discussed at the June and December modeling meetings (see also Response to Comment 405), and will be included in the text revisions. | Partitioning ($K_{OC}$ and $f_{OC}$) is presented throughout the related equations. Instances of<br>$\Theta + f_{OC} \times K_{OC} \times m$<br>should be replaced by<br>$\Theta \times (1 + a_{DOC} \times K_{OC} \times DOC) + f_{OC} \times K_{OC} \times m$<br><br>Categories: CFT Model<br>Sub-Categories: Partitioning, Fluff Layer |
| 542 | Appendix O | Specific | Section 2.1.1.2, page 6, Equation 3 | The equation should read<br>$\frac{\partial C_{T,F}}{\partial t} = \frac{\theta_F D_F}{\lambda H_F}(c_P - c_F)$<br><br>for the fluff layer and<br>$\frac{\partial C_{T,P}}{\partial t} = \frac{\theta_P D_F}{\lambda H_P}(c_F - c_P)$<br><br>for the surface layer of the sediment, or, if written in terms of total volume,<br>$\frac{\partial C_{T,F}}{\partial t} = \frac{\theta_F D_F}{\lambda H_T}(c_P - c_F)$<br><br>However, if the equation is written in terms of total volume, as currently presented in the text, then the equation creates mass transfer when there is no gradient in concentration. Assuming there is no concentration gradient,<br>$\left(\mathbf{C}_F = \frac{M_F}{V_F}\right) = \left(\mathbf{C}_P = \frac{M_P}{V_P}\right)$ | Region 2 raises two concerns with the fluff layer equations: 1) the denominator in the right-hand side should be $\lambda H_F$ instead of $\lambda^2$; and 2) "the equation creates mass transfer when there is no gradient in concentration." A revised derivation of fluff equations accounting for these comments will be delivered to Region 2 for review as part of the ongoing modeling interactions. In the interim, brief notes on each concern are provided:<br><br>1) As discussed with Region 2 during the June 28 and December 16, 2016 modeling meetings, the requested change to the mass transfer term will be made, i.e., replace $\lambda^2$ in the denominator of Equation (3) with $\lambda H_F$ (also Equation (8)). This will better mirror the mixing formulation of the underlying layers and address the concerns raised in Comment 543 regarding the time variation in the mass transfer rate (Equation (8)) that arises from the present formulation. | The CPG's revised write up of their derivation will need to be clarified. The discussion under the first bullet of sub-response #2 does not resolve the concern noted in the comment since the porosities for both layers are the same based on the CPG's assumptions. The revised write up and associated model code will be reviewed once provided and additional comments will be provided as necessary.<br><br>Categories: CFT Model<br>Sub-Categories: Fluff Layer |

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| | | | | and assuming that the parent layer is thicker than the fluff layer, $H_P > H_F$<br><br>then the equation as presented results in the following:<br>$\left(c_P = \mathbf{C}_P \frac{H_P}{H_P + H_F}\right) > \left(c_P = \mathbf{C}_F \frac{H_F}{H_P + H_F}\right)$<br><br>Please revise the text to correct the errors in this equation and the dependent equations (Equations 4 through 17). | 2) Region 2's concern regarding mass transfer in the absence of a concentration gradient is based on a misinterpretation of the variable usage in the text:<br>• The fluff and surface bed layer dissolved concentrations $c_F$ and $c_P$ appearing in Equations (1), (2), and (3) are not on a total volume basis. Rather, they are the dissolved concentrations within the pore spaces of each layer. So Region 2's definition of $c_F = \frac{M_F}{V_F}$ and $c_P = \frac{M_P}{V_P}$ is consistent with the draft RI text only if the V's represent pore space volumes (i.e., layer volume * layer porosity). In Equations (1) and (2), $c_F$ and $c_P$ are multiplied by porosities ($\theta_F$ and $\theta_P$) that are defined on a total volume basis ($AH_F + AH_P$) to yield the dissolved mass contribution to $C_{T,F}$ and $C_{T,P}$, which are the total chemical concentrations on a total volume basis. Thus, the terms can alternately be expressed as $c_F = \frac{M_F}{\theta_F V_T}$ and $c_P = \frac{M_P}{\theta_P V_T}$, and remain on a pore volume basis. See the Response to Comment 544 for additional discussion of the porosity terms.<br>• As a consequence of the above, Equation (3) yields no mass transfer in the case raised by Region 2:<br>$\left(\mathbf{C}_F = \frac{M_F}{V_F}\right) = \left(\mathbf{C}_P = \frac{M_P}{V_P}\right)$<br><br>In addition to clarifying variable definitions and implementing the change in Item 1 above, the revised derivation to be provided to Region 2 will express the mass transfer on a total concentration basis, as discussed with Region 2 at the December 20, 2016 modeling meeting. Overall these changes amount to maintaining Equation (9) using $\alpha = \frac{D_F}{\lambda H_F}$, i.e., changing the denominator in Equation (8) as per Item 1 above and setting $R_F = 1$. | |

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| 543 | Appendix O | Specific | Section 2.1.1.2, pages 5 through 7 | Equations 1 through 17 as presented are incorrect due to the errors in Equations 1, 2, and 3 noted in Comment Nos. 541 and 542 that were carried through the derivation of Equations 4 through 17. Fortunately the application in the code describing mass transfer between the fluff layer and parent bed is nearly correct. The only issue with the application of the derived equations in the code is that λ2 should be replaced with λ × HF. The error in the model code results in slower mixing between the parent bed and fluff layer. Assuming the rate of particle mixing between the fluff layer and parent bed should be at least as large as the rate of particle mixing between the top two layers of the parent bed, the particle mixing rate is underestimated by a factor of 3 when HF is at its maximum value (0.001 meter [m]) and HP is at its minimum value (0.005 m), and by a factor of 10,000 when HF is at its minimum value (0.000001 m) and HP is at its maximum value (0.02 m). Please correct this error in the code and revise the text accordingly.<br><br>Note that the semi-analytical solution approach presented may not be necessary for particle mixing if the minimum fluff layer thickness is increased slightly. If revised as discussed in the previous paragraph, this semi-analytical approach could also be used to represent diffusive mixing between the fluff layer and water column. The derivation can be greatly simplified by first computing the dissolved contaminant concentrations and then solving the diffusion equations. | See Response to Comment 542, specifically Item (1).<br><br>The CPG will maintain the semi-analytical solution in specifying the fluff-bed mass transfer because of the advantages it offers over a finite difference approach. The analytical solution allows the algorithm to function under a wide range of parameter values for calibration and sensitivity, including an infinite mass transfer rate (i.e., the equivalent of assuming no fluff layer). In contrast, a finite difference approach would run into numerical stability issues and likely require intractably small timesteps. The CPG agrees that the semi-analytical approach could in principle be modified to allow for flux to the water column, and may incorporate this update if advantageous relative to a finite difference treatment of the term. | As discussed during follow up meetings, the CPG must represent diffusive exchange between the water column and fluff layer, the fluff layer and the bed, and the water column and the bed when the fluff layer is not present. They must also represent particle mixing between the fluff layer and bed. The mixing between the fluff layer and the bed may be combined into a single solution, but the total mixing rate between the fluff layer (particle and diffusive) must be greater than or equal to the total rate of mixing between the top two layers of the bed (See comment 405).<br><br>Categories: CFT Model<br>Sub-Categories: Fluff Layer |
| 544 | Appendix O | Specific | Section 2.1.1.2.2, page 8, bullets | The porosity and OC content of the fluff layer are defined by the assumption that the fluff layer has the same properties as the surface layer of the parent bed (as stated in Section 2.1.1.1, page 4, seventh bullet). However, if the fluff layer porosity is not equal to the parent bed porosity, it should be greater than, not less than, the parent bed porosity. Similarly, the $f_{OC}$ in the fluff layer should be greater than that of the bedded sediments if the fluff layer includes solids entering from external sources where the more labile fraction has not yet decayed. That is, the water column $f_{OC}$ should be greater than the fluff layer $f_{OC}$, which should in turn be greater than the parent bed $f_{OC}$. Please revise the text to eliminate this section once the derivation of the fluff layer mixing equations is corrected in the model. | The reviewer misunderstands the assumptions listed on this page, and the text will be revised to be clearer. A brief clarification is provided in the interim:<br>• The concerns noted by the reviewer do not exist because the variables used in the derivation are defined on a total volume basis ($AH_T = AH_F + AH_F$) as stated at the top of page 6 and the middle of page 8. Each concern is discussed below.<br>• **Porosity:** The fluff layer and underlying sediment layer are assumed to have the same properties (stated in Section 2.1.1.1; as reviewer points out). This applies to the porosity as well and consequently it follows that physically "the volume of the pore space in the fluff is proportional to its thickness" (page 8). However, the porosities used in the fluff equations derivation are on a total volume basis (as stated at top of page 6) and the fluff and bed layer porosities are not equivalent on this basis: $\theta_F = \frac{AH_F}{AH_T}\theta_T$ and $\theta_P = \frac{AH_P}{AH_T}\theta_T$ where $\theta_T$ is the total porosity ($\theta_T = \theta_F + \theta_P$), i.e., the quantity that is assumed constant across the fluff and bed layers | The CPG's revised write up of their derivation will need to be clarified. The revised write up and associated model code will be reviewed once provided and additional comments will be provided as necessary.<br><br>Categories: CFT Model<br>Sub-Categories: Fluff Layer |

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| | | | | | (total pore space divided by total volume). These definitions are used to simplify the $\beta$ term in Equation (17).<br>• **Carbon content:** Likewise, the carbon content $f_{oc}$ is also assumed to be constant across the fluff and bed layers. This means that "[t]he concentration of organic carbon ($f_{oc} * m$) in the fluff layer and surface layer of the parent bed are proportional to the respective volumes in each domain" (page 6) **when** carbon concentration is expressed on a total volume basis like the solids concentration $m$ (per definition at top of page 6). The substitution in Equation (17), $f_{oc,F}m_F = \frac{AH_F}{AH_T} f_{oc,T}m_T$, follows from $f_{oc,F} = f_{oc,T}$ and $m_F = \frac{AH_F}{AH_T} m_T$, where $m_T$ is is the total solids concentration ($m_T = m_F + m_P$), i.e., the quantity that is assumed constant across the fluff and bed layers (total solids mass divided by total volume).<br><br>The revised text will more clearly define the variable convention to avoid confusion.<br><br>The reviewer asks that Section 2.1.1.2.2 be removed. The interpretation of $\beta = \frac{H_F}{H_P}$ is important to understanding the governing equation (Equation 9) and the analytical solution used in the code (Equations 15 and 16), and the adjustments that will be made in Response to Comment 542 do not impact the $\beta$ term. When revising text, CPG will consider whether a reorganization would help for clarity.<br><br>It is noted that the assumption of equal $f_{oc}$ in the fluff layer and bed layer will be maintained by analogy to the assumption of uniform bulk density (see Response to Comment 540). This was discussed during the December 16, 2016 meeting and deemed acceptable by Region 2 as part of modeling the mass transfer on a total concentration basis.<br><br>A revised derivation of the fluff mass transfer equations will be delivered to Region 2 for review as part of the ongoing modeling interactions. | |

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| 545 | Appendix O | Specific | Section 2.1.1.2.4, page 9, Equation 18 | Please clarify whether the value of $k_f$ used in the CFT model is the same as in the ST model. If not, please correct this inconsistency. | See Response to Comment 405.<br><br>As discussed with Region 2 during the June 28 and September 20, 2016 modeling meetings and outlined in the notes transmitted on June 27, 2016, the $k_f$ term is an important component of the CFT fluff layer algorithm but does not appear in the ST model. The term allows the CFT fluff thickness to adapt to changing shear stress conditions by moving thickness not actively undergoing erosion/deposition into the underlying layer (along with the associated contaminant mass). The lack of a $k_f$ term in the ST model is not an inconsistency, but rather reflects the different definitions of the fluff layer within each model. Whereas the CFT fluff layer represent the material "going up and down," the ST fluff layer captures the reservoir of material that *could* be resuspended before stiffer sediments are encountered. The ST model uses a complex layering scheme to distinguish layers of differing erodibility (i.e., active layer, fluff layer, transitional layer, depositional layers, and parent layers) and the number of layers interacting with the water column ("going up and down") depends on the shear stresses experienced. See June 27, 2016 notes for additional discussion.<br><br>As a consequence of the above and Region 2's acceptance of the CFT fluff layer thickness tracking (per Response to Comment 405), no changes to the model will be made in Response to Comment 545. | Given the interactions with the CPG at follow up meetings the response is accepted and the representation of the fluff layer in the model will be reviewed once updated model code, inputs, and outputs are provided.<br><br>Categories: Model Consistency, CFT Model<br>Sub-Categories: Fluff Layer |
| 546 | Appendix O | Specific | Section 2.1.3, page 10, last sentence (continued on page 11) | Please revise the text to clarify whether the removal of bed layers due to the bed elevation change applied at the beginning of each time chunk is equal to the change computed during that same time chunk in the ST model, or if there is a lag of one or more time chunks. In addition, please clarify whether the solids, POC, and contaminant loads are distributed uniformly over depth in the ST, OC, and CFT models, respectively. | The bed elevation change predicted by the ST model due to the navigation scour is applied to the same time chunk in the OC and CFT models. The clarification will be made to the revised RI Report.<br><br>The text will be revised to clarify the release of solids and POC; the existing text indicates that the scoured contaminant mass was "distributed uniformly over the water column" and the same assumption was made for solids and POC. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document, Model Consistency<br>Sub-Categories: Bathymetry, Text Clarifications |
| 547 | Appendix O | Specific | Section 2.1.4, page 11, fourth bullet | The model should not be used to predict changes at sub-grid scales, and unrealistically large reductions in concentration should not be associated with dredging small fractions of a grid cell. For grid cells that were capped at RM 10.9, the areal fractions addressed and corresponding concentration reductions should be presented in the RI Report. The same information should also be presented for the grid cells remediated as part of the alternatives presented in the FS Report. | As per Response to Comment 404, the CPG's position is that it is reasonable to represent a targeted remedy based on the expected concentration reduction aggregated to the model grid, and is awaiting feedback from Region 2 on the proposed approach that was presented at the April 27 mapping meeting. | See comments 403 and 404.<br><br>Categories: CFT Model<br>Sub-Categories: Remedial Benefit |

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| | | | | | The requested information on grid cells remediated during the RM 10.9 removal action and in FS alternatives will be provided in the revised RI and FS reports, respectively. | |
| 548 | Appendix O | Specific | Section 3, page 13, first paragraph, second sentence | Please revise the text based on the discussion of chemicals targeted for calibration in **Comment No. 372**. | Pending the outcome on Comment 372, the text will be revised accordingly. | See Comment 372.<br><br>Categories: CFT Model<br>Sub-Categories: CoPCs |
| 549 | Appendix O | Specific | Section 3.1.1.2, page 14, third paragraph, first sentence | The section of the navigation channel included in the harbor deepening project should be considered separately from the other portions of the channel. Please revise the model and the text accordingly, and incorporate additional sampling data as it becomes available (e.g., Newark Bay Phase III sediment sampling data). | The Newark Bay Phase III dataset is not expected to be available in time for use in the revised mapping, and Region 2 indicated at the September 20, 2016 meeting that the CFT model revisions for the LPR RI/FS need not incorporate this information. The topic of Newark Bay mapping was discussed briefly at the December 16, 2016 meeting, and Region 2 indicated that a Thiessen polygon approach was acceptable for the NBSA portion of the domain. Alternative treatment of the Harbor Deepening project area will be considered as a potential revision to the NBSA mapping, which the CPG anticipates discussing with Region 2 during the course of the ongoing modeling interactions. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: CFT Model<br>Sub-Categories: CoPC Mapping |
| 550 | Appendix O | Specific | Section 3.1.1.3.1, page 16, last paragraph, item 2c and footnote 6 | Please revise the text to provide an objective basis for choosing 30 ng/kg TCDD in the bottom segment of each core as the threshold for deep zeroing. This assumption eliminates the measured peak in 12% of the cores. The discussion in footnote 6 should also provide additional detail about the sensitivity analysis and its outputs. | The text will be expanded to provide additional details on the analysis and the selection of 30 ng/kg as a threshold. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 551 | Appendix O | Specific | Section 3.1.1.3.1, pages 16 through 17, and Section 3.1.1.4, page 18 | Please refer to on Appendix J. Please revise the text to provide a strategy to ensure that zeroed values are not spread laterally during interpolation to areas that should show contamination in the same depth interval of the sediment bed. | The text will be clarified. The zero concentrations below full inventory cores are not interpolated into areas with measured concentrations for that depth interval. The zero concentration in shallow sediment areas are limited to the spatial extent of the shallow sediment areas and are not interpolated beyond this extent. | Response accepted, pending review of the revised text.<br><br>Categories: CFT Model<br>Sub-Categories: CoPC Mapping |
| 552 | Appendix O | Specific | Section 3.1.1.3.2, page 17 | If areas of shallow sediment overlying hard bottom exist at a scale that is relevant to the associated grid cells and will not result in zeros interpolated to neighboring grid cells that do not have hard bottom, those areas should be identified and treated accordingly across all models. Areas of coarse material that may contain a smaller fraction of cohesive solids should not be treated the same as actual hard bottom. An initial check of the CPG ST and CFT models indicates that during the calibration period, no erosion into zeroed areas occurs, with the exception of two grid cells that erode more than 5.5 feet in the late 1990s. Although these cells are well upstream, near the Saddle River (grid indices I = 17 and J = 234, 236), and have low | Per the Response to Comment 551, the zero concentrations mapped in shallow sediment areas are not interpolated beyond these areas.<br><br>In Response to Comment 552, the CPG reported at the December 16, 2016 modeling meeting that a number of cells were changed to non-erodible below 6 inches in the ST model to be consistent with the shallow sediment areas defined in the contaminant mapping. This change was applied to grid cells for which 50% or more of the cell area overlapped with | Response accepted, pending review of the revised text.<br><br>Categories: Model Consistency<br>Sub-Categories: Bed Properties |

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| | | | | contaminant concentrations, they do represent a solids load that would dilute concentrations elsewhere in the domain. Please correct this instability in the ST model and adjust the bed properties for non-erodible locations consistently in all models. See Attachment 4, Figures 1a and 1b. | the mapped shallow sediment areas. Moreover, during the course of the meeting discussion, the Region 2 modeling team suggested that the upper few miles could perhaps be treated as non-erodible (hard bottom) as was done in the FFS/ROD ST model. As a consequence of this discussion and follow-up interactions with the Region 2 modeling team, the cells above RM 14.7 (including cells (17,234) and (17,236)) have been made non-erodible in the ST model. | |
| 553 | Appendix O | Specific | Section 3.1.1.5, page 18, and Attachment 1, Figures 13 through 22 | The text states that 2010 mapping was used for 1995 initial conditions outside the RM 1 to RM 7 reach. Model inputs received from the CPG in December 2014 indicate that there were a number of grid cells outside the RM 1 to RM 7 reach where sediment initial concentrations for 1995 were not equal to sediment initial concentrations for 2010 (See **Attachment 4, Figure 2**). Please expand the spatial extent shown on Attachment 1, Figures 13 through 22, to include the full 17-mile LPRSA and clarify the statement in Section 3.1.1.5 that contaminant initial conditions for locations outside of the RM 1 to RM 7 reach are identical for the 1995 and 2010 mapping. | The 1995 and 2010 mappings outside the RM 1 to RM 7 reach of the LPRSA are identical and thus no text revisions are required on this point. The noted concentration differences in the initial conditions between 1995 and 2010 were due to a glitch in input processing. These differences are mostly less than 0.1 ng/kg for 2,3,7,8-TCDD, with 5 cells having differences between 0.1 and 1 ng/kg. In the revised RI Report and model, there will be no discrepancies between the model initial conditions for the two periods outside of the RM 1 to RM 7 reach. The inputs and associated figures will be updated to reflect the revisions to the COPC mapping. | As discussed at the March 29, 2017 meeting, it may not be appropriate to assume that the sediment initial conditions outside of the RM 1-7 reach are the same in 1995 and 2010. This is particularly true in places that have significant changes in concentration during the CPG's long term calibration. The CPG stated that they will examine those areas more closely during their recalibration of the model, and address the disconnect between predicted and measured 2010 concentrations accordingly.<br><br>Categories: CFT Model<br>Sub-Categories: CoPC Mapping |
| 554 | Appendix O | Specific | Section 3.1.1.7, page 19 | Please revise the text to clarify why incomplete cores were not included in the calculations. The interpolated values in the locations and depths associated with those cores should be compared to the data that were excluded. In addition, in cases where a concentration of zero was assumed at the bottom of a core, please clarify whether the concentration profile above that point suggests that this is a reasonable assumption. | The text will be clarified as requested. For the layer below 5.5 feet, laterally interpolated values will be compared to measured data for cores that were excluded from the interpolation due to gaps between core segments. | CPG Response accepted, pending review of the revised text.<br><br>Categories: CFT Model<br>Sub-Categories: CoPC Mapping |
| 555 | Appendix O | Specific | Section 3.1.2, page 21, items 2 and 3 and Figures 3-8c and 3-8e | The text states that a long-term calibration run was performed to establish a surface mean-normalized bed shape (Figure 3-8c), which was then "applied to the initial sediment concentration to establish a vertical structure for the top 15 cm by multiplying the shape with the value from the surface mapping for the initial conditions of interest." Please revise the text to clarify how the shape shown in Figure 3-8c, which has a near-surface mean normalized 2,3,7,8-TCDD value of approximately 0.45, and the 0-15 cm average concentration for 2,3,7,8-TCDD of 3.58e-4 mg/L (printed on Figure 3-8d) resulted in the near-surface concentration of approximately 1.0e-4 mg/L shown in Figure 3-8d (rather than 1.61e-4 mg/L). | Because the mean profiles shown in Figures 3-8b, 3-8c, and 3-8d were calculated by averaging values from each grid cell, multiplying values shown in Figures 3-8b and 3-8c does not yield the values shown in Figure 3-8d (i.e., the product of averages is not equivalent to the average of products). The text will be revised to clarify that the concentration profiles shown in these figures represent averages of all cells within RM 1-7 of the LPR. | CPG Response accepted, pending review of the revised text.<br><br>Categories: CFT Model<br>Sub-Categories: CoPC Mapping |
| 556 | Appendix O | Specific | Section 3.1.2 | **Attachment 4, Figure 1** shows the vertical profile of initial conditions for a number of grid cells of interest. The first two cells (Figures 1a and 1b) discussed previously in **Comment No. 552** have excessive erosion; the next two cells (Figures 1c and 1d) show locations where contaminant concentrations were zeroed at depth; and the final two cells (Figures 1e and 1f) show extremely low concentration discontinuities in the profile of 2,3,7,8-TCDD concentrations. It appears that there is an issue in the vertical interpolation approach presented in this section of the report. Please | As discussed in the September 20 modeling meeting, the vertical interpolation approach is working as designed (details were provided in the notes delivered to Region 2 on September 18, 2016). It was agreed that the approach could be maintained provided that the initial vertical contaminant profiles are examined once other model revisions are complete (e.g., updated HST results and COPC mapping) and adjustments are considered to address large vertical | CPG Response accepted, pending review of the revised figures and associated description in the text. Note: the concentrantion inputs should have a reasonable floor based on the range of the measured data.<br><br>Categories: CFT Model<br>Sub-Categories: CoPC Mapping |

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| | | | | correct the error that generates these discontinuity errors, revise the text to reflect those corrections, and provide figures displaying the vertical profile used in the model along with the data used to generate the profiles for the locations with discontinuities, locations with erosion in excess of 15 cm, and locations zeroed at depth. | discontinuities (excepting grid cells with low contaminant concentrations). This position was confirmed with Region 2 during the December 16 modeling meeting. | |
| 557 | Appendix O | Specific | Section 3.4, page 25, last paragraph (continued on page 26) | While the cited references argue for reducing the transfer of contaminants from resuspended sediments, none suggest that eliminating that transfer is appropriate. The site-specific high-volume (HV) CWCM field data include measurements of both the dissolved and particulate fractions, which reflect the degree of equilibrium for these two phases while the measured solids are suspended in the water column. The phases other than the detrital POC-bound carbon (freely dissolved, DOC, and algal POC-bound) would have longer residence times in the water column and are likely to be much closer to equilibrium with each other. Please revise the model and the corresponding text to represent partitioning to each of these four phases in a way that is consistent across models and appropriately reflects the total dissolved and total particulate HV-CWCM data, the bioavailable freely dissolved fraction, and the algal POC-bound fraction. Please refer to Attachment 6. | See Response to Comment 371 regarding the revised partitioning approach that is being discussed with Region 2. The corresponding text modifications will be made to the revised RI Report. | See comment 371. The CPG's proposed approach does assume that the freely dissolved, DOC bound, and Algal carbon bound fractions are in equilibrium with each other.<br><br>Categories: CFT Model<br>Sub-Categories: Partitioning |
| 558 | Appendix O | Specific | Section 3.5, page 30, second paragraph, last sentence (continued on page 31) | A purely non-cohesive fluff layer should not exist. Please correct this by using the ST model fluff layer results and revise the text accordingly. | As described in the footnote on this page, a correction has been implemented within the CFT model to handle the rare case mentioned in the text. As such, it will not affect future model simulations and the text will be revised accordingly.<br><br>It is noted that, as discussed with Region 2 in recent modeling interactions, the CFT model's fluff layer tracking will be maintained (see Responses to Comments 381 and 405) along with the assumption of uniform bulk density across the fluff layer and underlying surface layer in representing the contaminant mass transfer between these layers (per Response to Comment 540). | The revisions to the fluff layer will be reviewed when updated code, inputs, and outputs are submitted to EPA.<br><br>Categories: CFT Model, Model Consistency<br>Sub-Categories: Fluff Layer |
| 559 | Appendix O | Specific | Section 3.6, page 31, second paragraph, first sentence | Please revise the text to clarify how all of the particle mixing processes noted in the previous paragraph are represented in the model if the depth of sediment mixing represented in the model is "due to bioturbation alone." In addition, please clarify what datasets were used to determine the distribution of benthic biomass over depth. | The particle mixing is the result of all processes of which bioturbation is generally viewed as the most significant for sub-surface sediment. The text will be revised to reflect the basis for the mixing depth within the revised model, incorporating any adjustments made during the calibration. | CPG Response accepted, pending review of the revised text.<br><br>Categories: RI Document, CFT Model<br>Sub-Categories: Text Clarifications, Sediment Mixing |

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| 560 | Appendix O | Specific | Section 3.6, page 32, last paragraph | As noted in Appendix K (Section 5.3.2.2, page 26, second paragraph, last three sentences), the calibrated mixing profile was not applied uniformly due to an error that affected approximately 12% of the LPR cells. Please revise Section 3.6 to be consistent with the description in Appendix K. The CPG CFT model simulation for the combined long- and short-term calibration periods takes under a week to complete. Given this short time required to correct the error, model results should be corrected rather than presenting results for a model with known errors in the RI Report. | This will be addressed in the revised RI Report, incorporating any changes made during the calibration of the revised model. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: CFT Model<br>Sub-Categories: Calibration |
| 561 | Appendix O | Specific | Section 4.1.1, page 34, first paragraph, second sentence | The description in the text suggests that the data were matched in space by grid cell, and that 20% of the data and model results were then discarded and the remaining ranked results compared for the eight remaining 10-percentile bins within a given RM. Please clarify the description if this is not the case. Otherwise, please revise the text to present the justification for discarding 20% of the data. In addition, this does not appear to be an appropriately rigorous quantitative test for a model that is being proposed for use in assessing a remedy at a resolution finer than individual grid cells and targeting the highest 25% of the data across the entire study area (approximately 15% of the area) and the highest 19% of the data across the RM 1 to RM 7 reach (approximately 15% of the area). Please revise the model and the corresponding text to include quantitative calibration metrics that provide insight into the model performance at scales relevant to the remedial alternatives that will be proposed in the FS. | The quantitative metric used in Figures 13 of Appendix K is designed to assess model-data agreement while minimizing potential bias from extreme data points (it is also used in Figure 9 for the water column data). All data and model results were presented in the figure. The extreme 20% (10% in each tail) of the data and model results were excluded in the calculation of the statistic such that these less frequently observed concentrations do not control the overall statistic. The statistics posted in Figures 9 and 13 were used to judge the best fit calibration. Note this is just one of the many metrics to assess the model performance and to support the CFT model's use in FS alternatives evaluation.<br><br>A revision of this metric will be considered when providing updated calibration results to Region 2. Please also see Response to Comment 403. | See comments 403 and 404.<br><br>Categories: CFT Model<br>Sub-Categories: Calibration Metrics |
| 562 | Appendix O | Specific | Section 4.2, page 38, second paragraph, first sentence, and Figures 4-4a and 4-4b | An analysis of the CPG's hypothesis, that the lack of contaminant partitioning to non-cohesive solids results in an underprediction of concentrations in sandy, depositional areas, was explored considering the top panel of Figure 4-4a. Because initial condition data are not available for the rest of the 17-mile LPRSA (upstream of RM 7 and downstream of RM 1), those locations were not considered in the analysis performed for this review. Attachment 4, Figure 3 presents the CPG's figure reproduced from the model outputs that the CPG provided to EPA in December 2014. The figure presents the 2,3,7,8-TCDD concentrations in the top 15 cm of the sediment plotted against the model cohesive fraction for all cells between RM 1 and RM 7 with deposition greater than 0 cm. The red points on this figure generally reproduce the model results presented on the top panel of Figure 4-4a, with some unexplained exceptions.<br><br>Attachment 4, Figure 3 is repeated in Attachment 4, Figure 4 with model results for three points in time: the CPG initial conditions, the end of the long-term calibration, and the start of the short-term calibration. For all three time horizons, all of the model grid cells between RM 1 and RM 7 with predicted deposition of 0 cm or greater between 1995 and 2010 are plotted. Note that the blue diamonds representing the model initial | See Response to Comment 373. The CPG will consider the factors Region 2 raised in this comment and in Comment 535 when re-assessing the performance of the updated model. As discussed during the June 28, 2016, the request on analyzing composition change in deposition areas will be addressed as needed using the refined CFT model results coupled with the updated HD/ST/OC models.<br><br>Note that the CPG has confirmed the information shown in Figure 4-4a is correct. The three red open circles (i.e., (18,133), (19,131), and (17,133)) correspond to cells that experienced net erosion between 1995 and 2010, and thus those cells were not included in this figure. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: CFT Model<br>Sub-Categories: Partitioning |

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| | | | | conditions do not include any values for cohesive fraction less than 35%, and the model does not show the relationship between cohesive fraction and concentration observed in the data because the blue diamonds do not extend into the lower fraction cohesive range. After the long-term calibration period, the model, represented by the red squares, develops a number of non-cohesive cells and a relationship between contaminant concentration and fraction cohesive. Finally, the initial condition for the short-term calibration and projections, represented by the green triangles, shows no relationship between cohesive fraction and concentration.<br><br>Additional concerns arise upon taking a closer look at Figure 4-4a. If the same figure is generated for the cells that have a fraction cohesive of less than 40% at the end of the long-term calibration, all of those cells started with significantly higher cohesive and 2,3,7,8-TCDD concentrations. After 15 years of simulation, the model reproduces the general shape of the data in the top panel of Figure 4-4a. In addition, the model contaminant concentrations show no clear relationship with fraction cohesive above a fraction cohesive of 60%, which represents more than 60% of the depositional grid cells. This suggests that underprediction of contaminant concentrations in cells experiencing non-cohesive deposition could only be an issue in not more than 40% of the grid cells; however, this underprediction issue extends to a larger fraction of the CFDs presented in Figure 4-3. Please verify that the predicted change in composition is supported by the data and that the data presented are in depositional areas based on the bathymetry data and not the model.<br><br>The short-term model calibration initial conditions should reflect the observed relationship between cohesive fraction and contaminant concentration shown in Figures 4-4a and 4-4b. By replacing the lower concentrations associated with non-cohesive sediments at the end of the long-term calibration (Attachment 4, Figure 5, red squares) with elevated concentrations at the beginning of the short-term calibration (Attachment 4, Figure 5, green triangles), the concentration would once again approach the previously predicted shape overpredicting the rate of recovery for the period after 2010. A slight shift in the composition, an increase in the detrital POC partition coefficient, and/or additional resuspension in erosional areas may produce large changes in the predicted contaminant concentration values. Additional analyses should be presented to support the argument that the existing CFT model framework cannot be used to reproduce the data, and if necessary the framework should be modified to handle partitioning to non-cohesive particles. | | |

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| 563 | Appendix O | Specific | Section 4.2, page 38, third paragraph and Section 4.2.1, page 39 | Carroll et al. (1994) reports the fOC on particles of different sizes in the Hudson River. The fOC values of 6% and more on particles larger than 293 µm (Carroll et al. 1994, Table 1) make it unlikely that these were sand particles with OC coatings. Di Toro et al. (1991) summarizes data from Prahl (1982), including measurements of fOC on sand-sized particles, which were segregated based on density. Sand-sized particles with densities greater than 1.9 g/cm3 had an fOC of between 0.2% and 0.4%, while the fOC of lower-density sand-sized particles exceeded 10%. It would be inconsistent to model the transport of low-density, high-fOC particles using the non-cohesive transport equations in the ST model because these lower-density particles would not be transported as bedload in the same way as the sands that are represented in the ST model non-cohesive solids classes.<br><br>The vast majority of non-cohesive solids transport in the LPR is present as bedload rather than suspended load, and adding bedload to the CFT model would require additional model development and necessitate smaller time steps due to the faster settling velocity of the non-cohesive particles. If this process is important, as suggested in the text, consideration should be given as to how partitioning to sands could be incorporated into the model and represented appropriately without resulting in unacceptable simulation times. Refer to Comment No. 562 for additional concerns with the CPG's analysis to support partitioning to cohesive solids. Please propose a solution to address these concerns with the representation of partitioning within the CFT model and revise the text to reflect any actions taken to revise the model. | See Responses to Comments 373 as well as 371 and 386. | Based on discussions during follow up modeling meetings, the CPG, they will check to see if their concern about the potential importance of contaminant transport associated with non-cohesive solids remains once they have recalibrated the model. Both the EPA team and the CPG agree that it is not desirable to add partitioning to non-cohesive solids.<br><br>Categories: CFT Model<br>Sub-Categories: Partitioning |
| 564 | Appendix O | Specific | Section 4.2.2, pages 39 through 40 | The analysis presented in this section attempts to estimate the contaminant concentrations that would have been calculated if partitioning to non-cohesive solids were included in the model. The results presented are computed by adding contaminant mass to the sediment bed. The contaminant concentration on non-cohesive solids is assumed to be proportional to the concentration on cohesive solids. This approach is not reasonable because the CFT model results are based on initial conditions and external inputs that already account for the total contaminant mass, and the increment added to represent contaminants sorbed to non-cohesive solids represents an artificial source of new contaminant mass. Please revise this analysis to eliminate the invalid creation of contaminant mass. | The evaluation was included simply to provide an approximate sense of the potential importance of non-cohesive solids on model predictions; the CPG did not suggest that this approach would be used as a surrogate model prediction moving forward.<br><br>As discussed in the Response to Comment 373, the potential influence of non-cohesive solids on predictions will be reassessed once other model revisions have been made. If an analysis of the type referenced in Comment 564 is invoked in the revised RI Report, the analysis and/or accompanying text will be adjusted to address Region 2's noted concerns. | Based on follow up modeling meetings, the CPG will check to see if their concern about the potential importance of contaminant transport associated with non-cohesive solids remains once they have recalibrated the model. Both the EPA team and the CPG agree that it is not desirable to add partitioning to non-cohesive solids.<br><br>Categories: CFT Model<br>Sub-Categories: Partitioning |
| 565 | Appendix O | Specific | Attachment 1, Figures 3 through 22 | Please adjust the figure scales so that lower contaminant concentrations can be distinguished from zeroed areas. Please include additional breaks that distinguish zero separately, and add greater resolution at the low end of the scale. The scale should include breaks for relevant human health and ecological risk screening values and the estimated background concentration. | A break for zeroed values and one additional break in the lowest bin will be added to the maps in response to the request (e.g., 100 ng/kg 2,3,7,8-TCDD). Additional breaks will not be added to the color scale, so as to maintain a clear presentation in which colors are easily differentiated. | The requested changes to the color scale must be made to distinguish between areas that presently fall below or exceed relevant concentration thresholds. If necessary resolution at the high end of the scale can be sacrificed. |

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| | | | | | | Categories: RI Document<br>Sub-Categories: Figures, Text Clarifications |
| 566 | Appendix P | General | | It was inferred that the "Bioaccumulation Model Calibration Report," delivered to EPA on July 2, 2015, is Appendix P of the Draft RI Report. This document should be labeled correctly as Appendix P – Bioaccumulation Model, and the appendices delivered with this document should be labeled as attachments consistent with the approach used for the other Draft RI appendices. | Document classification and labeling will be updated as requested. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 567 | Appendix P | Specific | Section 2.3, page 4, second paragraph, third sentence | It is not necessarily true that "the mechanistic model represents a more realistic picture of bioaccumulation." Even if all of the mechanistic processes are contained within a model, if the rates governing those processes are not properly set, a mechanistic model can provide less accurate predictions than a simpler empirical model would. Please remove this statement from the document. | Text will be revised as requested. | Response accepted, pending review of the revised text.<br><br>Categories: RI Document<br>Sub-Categories: Text Clarifications |
| 568 | Appendix P | Specific | Section 3.1.4, page 11, Figure 3-3 | The fish feeding guild classifications presented in Figure 3-3 are not consistent with the dietary preferences of bass in the draft bioaccumulation model presented to EPA. In the model, bass are set to consume an equal amount of planktivores as small forage fish (each category represents 40% of their overall diet), whereas the data shown in Figure 3-3 indicate that there are 11 times as many small forage fish and "invertivores/omnivores" as planktivores (88% vs. 8%, respectively) in the LPRSA. Please correct the bass dietary preferences in the model. | See Response to Comment 383. Feeding preferences will be reviewed and updated as needed. Sensitivity of model performance and projections to alternative assumptions about feeding preferences will be discussed. | Comment 383 is not entirely relevant. This is a separate point regarding fish feeding preferences that will need to be evaluated separately by EPA when CPG completes their response and model updates.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Feeding Preference |
| 569 | Appendix P | Specific | Section 3.1.5, page 12, second paragraph | The particulate ventilation construct is an empirical "black box" used to make the carp model fit the data. A chemical-specific particulate ventilation parameter is especially inappropriate as there is no basis for assuming different ventilation rates for different chemicals. Carp bioturbation is, however, known to stir up deeper and more contaminated sediment, up to 16.7 cm deep. Huser et al. (2015) determined that "the sediment mixing depth was at least 2.5 times greater in areas with carp (13.0 ± 3.7 cm) than in areas from which carp had been excluded (5.0 ± 1.2 cm) using exclosures." Please recalibrate the model without the particulate ventilation construct, but including carp feeding on deeper sediments, and revise the text accordingly. | The CPG will evaluate whether additional work will be done to better document the rationale for the inclusion of the particulate ventilation constant. The inclusion of respiratory uptake pathway is supported by the available evidence. It is important to recognize the role that ventilation plays in the uptake of contaminants from the LPRSA by carp. The uncertainties associated with this exposure pathway will be better characterized in the revised report and alternative explanations for the relatively high tissue concentrations in carp will be considered. | In the 8-28-2016 bioaccumulation meeting, R2 showed that particulate ventilation was not required for carp modeling at other sites including Lower Fox River (carp was a calibration species), Housatonic River (carp was a validation via surrogate species), and Hudson river (bullhead was used as representative benthic species and has higher contamination than carp.) CPG must clearly explain why this site would be unique with respect to this construct if EPA is to accept a model with this construct within it.<br><br>CPG is directed to remove the particulate ventilation construct from their bioaccumulation model.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Particulate Ventilation |
| 570 | Appendix P | Specific | Section 3.2.1, page 14, last bullet | The text lists "Near-bottom particulates" as a physical medium for inclusion in the bioaccumulation model. This is later defined (in the last bullet on page 16) as particulate matter in the bottom layer of the water column, rather than the modeled "fluff layer." It is unclear why the fluff layer, which was added to the CFT model, is not being used in the bioaccumulation | Feeding preferences of benthic organisms will be reevaluated and refined as will the model parameters (e.g., "near-bottom particulates" and "fluff layer") used to represent the food consumed by benthic invertebrates. A filter feeder guild will be added to the model. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review. |

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| | | | | model by organisms feeding on that layer. Please revise the text to clarify why, in a mechanistic model where the layer used for feeding by a given organism is explicitly modeled, the water column above that layer is considered a better representation of contaminant concentrations. | | Categories: Model Consistency<br>Sub-Categories: Fluff Layer |
| 571 | Appendix P | Specific | Section 3.2.2, page 16, first bullet, first sentence | Using the top 2 cm of the sediment as the physical exposure medium is not acceptable. The available data to constrain predicted concentrations within this layer (water column concentrations and 15-cm concentrations) are not direct measurements of 2-cm concentrations. Furthermore, it appears likely that multiple alternative calibrations within these constraints are possible (e.g., higher or lower 2-cm sediment concentrations could be calculated and the model could still be predicting reasonable water column and 15-cm concentrations). Please recalibrate the model using a different physical exposure medium for sediment and rewrite this section accordingly. | The CPG strongly disagrees with this comment; however, the top 15 cm will be used to represent the physical exposure media for deposit feeders, as directed by Region 2 in the exposure depth dispute resolution. Sediment profile imaging data will be used to estimate the biomass fractions exposed to depth intervals across the top 15 cm. | This issue has been resolved via dispute resolution. EPA will verify the model is recalibrated to appropriate exposure depth.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Sediment Exposure Depth |
| 572 | Appendix P | Specific | Section 3.2.2, page 16, first bullet, last sentence | The depth of mixing, the rate of mixing, and the depth of exposure are not directly constrained by the site-specific data collected for the RI (refer to **Comment Nos. 535, 538, 559, 560, 569, and 611**). The CPG should present CFT sensitivity analysis results for a range of mixing depths and rates, and the depth of exposure should be revised to reflect the depth determined as a result of the currently ongoing dispute resolution. The final mixing depth, mixing rate, and vertical variation in particle mixing rate should reflect the results of the dispute resolution as well as the sensitivity analysis. This sentence should be revised to reflect the resulting changes in the CFT calibration and exposure depth. | The CPG will evaluate the sensitivity of the calibration to alternative assumptions about the physical exposure medium and adjust the calibration accordingly, if warranted based on the expanded evaluation. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: CFT Model<br>Sub-Categories: Sediment Mixing |
| 573 | Appendix P | Specific | Section 3.2.3, page 17, item 4 and second paragraph, first sentence | The analysis of relative abundance of the three benthic invertebrate groups, detailed in Appendix E, is based on combining non-site-specific species weights from Chesapeake Bay with local counts. This is a flawed analysis as discussed in the comments for Appendix E. Furthermore, the conclusion that "DETs comprise the majority of the benthic biomass in the LPRSA" is based on a highly variable and uncertain set of biomass data, especially for *Corbicula sp.* (Asian mud clam). For RM 4 to RM 13, the data suggest that detritivores and deposit feeders are roughly equal in biomass, as presented in the analysis in **Comment No. 614**. | The CPG disagrees with Region 2's contention that the analysis is flawed; however, the benthic organism biomass will be recalculated to: 1) include new biomass data found during a literature review; and 2) omit corbicula shell weights. Species' assignment to feeding guilds and data on specific feeding behaviors for abundant species (on a biomass basis) will be discussed. | The analysis errantly included shell weight in bioavailable prey data which is the basis for the "flawed" label. It also did not account for significant differences by river mile (or river reach). Even a corrected analysis would be subject to extensive uncertainty due to the use of off-site biomass data and site-specific differences in biomass. Model feeding preferences for benthic invertebrates must be recalibrated and uncertainty in these assignments evaluated.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Benthos, Spatial Variability |

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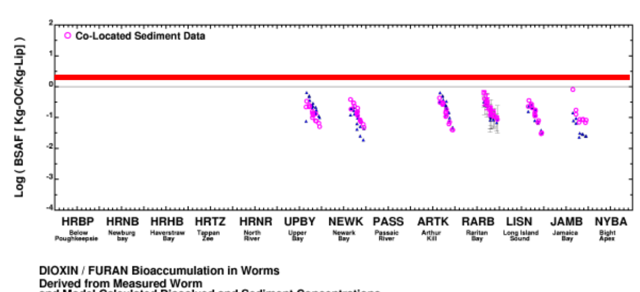
| 574 | Appendix P | Specific | Section 3.2.5, page 21, first paragraph, first sentence | The plots presented in Attachment 4, Figures 6a through 6e, developed by HDR show 2,3,7,8-TCDD fish tissue data by RM for forage fish, carp, white perch, American eel, and bass. As illustrated by these plots, for many species there are strong relationships between 2,3,7,8-TCDD concentration and RM, which makes the use of a single "modeling area" for each species inappropriate. Within the attached figures, central tendencies used in the CPG's modeling are plotted as horizontal lines.<br>As stated in the RI Report Executive Summary (page ES-4, third paragraph, first sentence), sediment concentrations are also heterogeneous and have a relationship with RM ("High surface sediment 2,3,7,8-TCDD concentrations are rare upstream of RM 12…"). Choosing not to calibrate the bioaccumulation model with RM bins results in the elimination of valuable information and unnecessarily simplifies the modeling, producing a model that is unacceptable for risk evaluation. Please revisit the model calibration using RM bins or another appropriate spatial segregation and reproduce this section of the report accordingly. | The bioaccumulation model will be partitioned and calibrated for estuarine, transitional and freshwater reaches. The transitional reach might be further subdivided, if warranted based on changes in food web structure across the transitional reach. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration, Spatial Variability |
| 575 | Appendix P | Specific | Section 3.3.1, page 31, Table 3-3 | The statement that "CYP450 1A expression (CYP450 1A1 is the most important enzyme in TCDD metabolism for vertebrates) is not known to occur in benthic invertebrates" suggests that using fish-derived 2,3,7,8-TCDD metabolism rates for invertebrates results in higher metabolism estimates than would be expected based on known enzyme metabolism processes. However, even when calibrating the model using these high metabolism values, the CPG model for deposit feeders is significantly overpredicting tissue concentrations, as evidenced by Figure 10-16 of the cited HydroQual (2007) document, reproduced below. The CPG model produces biota-sediment accumulation factors (BSAFs) for worms (and worm contaminant concentrations) that are much too high. The CPG's Log BSAFs for worms (0.34 to 0.41 Log kg-OC/kg-Lip) are plotted as a red line above the Log BSAFs observed and modeled in the New York-New Jersey Harbor Estuary in the figure below. The BSAFs predicted by the model are an order of magnitude higher than those observed. Other lines of evidence regarding the overprediction of contaminant concentrations in benthic invertebrates are presented in **Comment No. 595**. Please revise the bioaccumulation model such that the BSAFs for benthic invertebrates are in line with observed data.<br><br><br>Figure 10-16. Lipid/organic carbon-normalized BSAFs for dioxin and furan congeners in worms. | The BSAF for deposit feeders (approximately 1) is in line with observed site-specific data. Windward can include BSAFs observed and modeled in the New York/New Jersey Harbor Estuary into its analysis of benthic invertebrate SPAFs, if provided with the data that the CPG requested in its comments on the FFS and in a subsequent FOIA request, which Region 2 has refused to provide, and if those data are suitable for calculating BSAFs. | EPA disagrees with CPG calculation of a BSAF of ~1.<br><br>The deposit feeder BSAF is >2.0 as shown in the following calculation:<br><br>The BSAF is defined (Ankley et al., 1992) as<br>$BSAF = \frac{C_o/f_\ell}{C_s/f_{soc}}$<br><br>From the delivered model inputs<br><br>$C_O$ = DEP (4) = 0.38 ug/kg **(2378-TCDD)**<br>$f_l$ = Lipid Fraction of Organism = 2%<br>$C_s$ = Conc. in Sediment Solids = 0.46 ng/g<br>$f_{soc}$ = OC content of sediment = 0.057 (5.7%)<br>**Deposit Feeder BSAF = 2.35** -- Log BSAF = 0.37<br>All other spatial averaging areas, BSAF > 2.0<br><br>CPG must provide the numbers for their BSAF 1.0 calculation.<br><br>At the 8-28-2016 bioaccumulation model, John Toll agreed to reduce the deposit-feeder BSAF by a factor of 2. CPG must deliver a model that produces a predicted deposit-feeder BSAF of approximately 1 or lower. |

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| | | | | | | Categories: Bioaccumulation Model<br>Sub-Categories: Benthos, Calibration |
| 576 | Appendix P | Specific | Section 3.3.1, page 31, Table 3-3 | The rationale for benthic invertebrate metabolism of 2,3,7,8-TCDD states that the nature of metabolism of 2,3,7,8 TCDD is unknown and that there could be inefficient transfer of dioxin/furan congeners from sediment or that worms may be able to metabolize dioxin. In other words, the processes are highly uncertain, which calls into question the choice of a mechanistic model for these benthic compartments. Furthermore, no comparisons were made between tissue data and model results within the model calibration. The $K_M$ parameter was used as an open parameter due to process-level uncertainty, but whether this parameter was properly selected is not possible to assess without comparing the model results to benthic data. As discussed in **Comment Nos. 575 and 595**, it appears that the benthic model is dramatically overpredicting contaminant uptake for deposit feeders. Please consider revising the model calibration to utilize a BSAF for this model category using either data available from HydroQual (2007), cited within the report, or site-specific data from the worm bioaccumulation tissue tests, especially if the mechanistic model cannot be revised to produce reasonable BSAF predictions. | See Response to Comment 575. | As discussed in EPA's response to comment 575, EPA maintains that the benthic model for deposit feeders as delivered was significantly overpredicting and needs to be recalibrated.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Benthos, Calibration |

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| 577 | Appendix P | Specific | Section 3.3.1, page 33, Table 3-4 | The range of RMs contained within each calibration domain is inappropriate given the gradient in contamination acknowledged within the RI Report. As stated in the RI Report Executive Summary (page ES-4, third paragraph, first sentence), "High surface sediment 2,3,7,8-TCDD concentrations are rare upstream of RM 12…" In all three of the RM bins presented in Table 3-4, concentrations upstream of RM 12 are averaged with data from between 5 and 12 RMs downstream of RM 12.<br><br>Furthermore, the CPG seems to have conflated the concept of "species range" and "home range" within this analysis, modeling each entire "species range" as a single "box." To define the appropriate range for each species would involve a combination of species-specific "home range" information and information about differing sediment concentrations. Furthermore, even if a species has a relatively wide home range, animal tissue data suggest that these species are sensitive to the zone in which they are caught (refer to the data presented in Attachment 4, Figures 6a through 6e). Therefore, looking at the trends in tissue data can help to define the bins in which the animal tissue data should be compared to model predictions.<br><br>The CPG's research regarding the "home range" for each species and how this translates into the resulting assumptions regarding the exposure range that should be applied to each organism should be clearly presented in this report. Given the strong trends by 2-RM bins for many species (especially for 2,3,7,8 TCDD), the default approach should be to present model-to-data comparisons on this basis, unless the CPG is able to make a case that wider bins are appropriate due to a lack of significant differences in the data.<br><br>Please recalibrate the model using multiple sediment bins with greater spatial refinement for each calibrated organism to provide a more accurate and tightly constrained model, and revise the text accordingly once this recalibration is performed. | See Responses to Comments 408 and 574. | Changes to the model and its documentation will need to be thoroughly evaluated by R2 to assure that they are adequate and appropriate<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration, Spatial Variability |

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| 578 | Appendix P | Specific | Section 3.3.1, page 34, second paragraph, first sentence | The addition of an empirical "species-specific particulate ventilation constant" to the bioaccumulation model for carp indicates that the carp model is not working without this factor and provides another open parameter for what is already seen as an unconstrained model. This parameter seems to be unique to this model, as EPA's team has not encountered a "particulate ventilation factor" in any other bioaccumulation modeling exercise. Furthermore, the use of a different factor for different chemicals loosens any constraint on the carp calibration. Given that only a single model-to-data comparison is used for each organism, carp can be successfully calibrated to any exposure scenario simply by changing the input for each chemical being modeled.<br><br>The fact that carp are not accumulating enough contaminant in the model without this empirical factor likely means that carp are being exposed to more contaminated, deeper sediment than what is being modeled by the CPG. Alternatively, the process of carp feeding may be stirring up more contaminated, deeper sediment that is not appropriately accounted for in the shallow water predictions. Huser et al. (2015) indicates that carp can stir up sediment as deep as 9.3 to 16.3 cm below the surface.<br><br>The use of the empirical "particulate ventilation constant" acknowledges that carp will stir up sediment and have a concurrent effect on sediment remobilization. In the model, however, this affects the bioaccumulation prediction for carp alone, with no effect on water column concentrations or concentrations predicted in any other organism. In addition, this empirical factor, for which the CPG acknowledges "no information is available regarding the correct parameter value," allows the CPG to simply increase the amount of contaminant that carp are receiving from the water column, whether feeding preferences are properly set or not (refer to **Comment No. 569**).<br><br>Finally, most bioaccumulation models assume that chemicals sorbed to POC and even chemicals sorbed to DOC are not bioavailable through the gills. For example, Arnot and Gobas (2004) state that "If associated with particulate or dissolved organic matter, the chemical is believed to be unavailable for uptake via diffusion into organisms." Therefore, stretching the model to assume that chemicals sorbed to POC are bioavailable through the gills represents a significant departure from accepted bioaccumulation modeling practice.<br><br>Please either recalibrate the model without this particulate ventilation construct or provide sufficient literature or experimental data to support this novel modeling approach. | Contamination via respiration during feeding is an important exposure pathway for carp.<br>See Response to Comment 569. | EPA disagrees that contamination via respiration during feeding has been proven to be an important exposure pathway for carp. Based on EPA response to comment 569 models at other sites have not required this pathway and have been successfully calibrated for carp. The proposed uptake multiplier is entirely unconstrained and could be used to calibrate the carp model regardless of underlying problems with the model. There is no literature basis, in terms of peer-reviewed bioaccumulation models, for this construct.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Particulate Ventilation |

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| 579 | Appendix P | Specific | Section 3.3.2, pages 36 and 37, Table 3-8 | Table 3-8 and the derived dissolved oxygen (DO) saturation averages shown at the bottom of this table raise the question as to why a single study area-wide or habitat-wide calibration statistic was used for all modeled organisms when there are RM-specific differences in DO saturation (as well as $f_{oc}$, contaminant concentrations, and fish tissue concentrations). For example, the study area-wide average DO saturation of 80% shown in Table 3-8 is unlikely to accurately reflect conditions in a river in which DO saturation is 97% at RM 14 and 66% at RM 1. Please recalibrate the model using greater spatial refinement and revise the text accordingly once this recalibration has been performed. | The requested change will be made. | Response accepted, pending review of the revised text.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration, Spatial Variability |
| 580 | Appendix P | Specific | Section 3.3.3, page 39, Table 3-9 | It is likely that the dietary absorption efficiency (AE) of non-lipid organic carbon (NLOC) needs to be set lower for benthic invertebrates given the significant overprediction of the model for this category. As discussed in **Comment Nos. 575, 576, and 595**, the BSAFs are too high. The unconstrained metabolism rate chosen for 2,3,7,8-TCDD in invertebrates must also be considered when setting this parameter, as these two parameters affect the same outcome; therefore, the AE cannot be calibrated without also reconsidering the calibration of the metabolism rate. Please either recalibrate or replace the benthic organism model with these considerations in mind. | Dietary absorption efficiency of NLOC and metabolism will both be revisited as part of the recalibration process. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration |
| 581 | Appendix P | Specific | Section 3.3.3, page 39, Table 3-9 | Using literature-based weights rather than site-specific weights for benthic invertebrates is not defensible as the CPG previously made the case, during the February 6, 2015 meeting, that benthic organisms are unique in the LPR and that phenotypic differences make them much smaller than in other locations. At a minimum, the CPG must acknowledge the uncertainty in this parameter and add an acceptable range within Table 3.9. Furthermore, the CPG should test assumed organism weights as part of the model uncertainty analysis, including the assumed benthic organism composition for predators (currently assumed to be dominated by detritivores). | Site-specific data will be considered when determining model estimates of weights for benthic invertebrates. | EPA questions whether site-specific biomass data are available. Please clarify if there are site specific benthic organism weight data, and provide the requested analyses and revisions to the text and tables.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Benthos |
| 582 | Appendix P | Specific | Section 3.3.3, page 41, Table 3-9 | Carp lipid fraction and fraction of porewater ventilated were set to their nominal values in the model calibration. Rather than adding an empirical "particulate ventilation constant" to the model because this compartment would not calibrate using these nominal values, the CPG should first attempt to calibrate this species using the uncertainty within the parameters for the existing peer-reviewed Arnot and Gobas (2004) model. It is likely that increasing the depth of sediment exposure for carp would improve the calibration. Please recalibrate the model using different values for the carp parameters and without the use of the "particulate ventilation constant," and revise the table and text as necessary based on this recalibration. | See Response to Comment 569. | EPA disagrees that contamination via respiration during feeding has been proven to be an important exposure pathway for carp. See EPA response to 569 and 578.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Particulate Ventilation |

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| 583 | Appendix P | Specific | Section 3.3.3, page 44, Table 3-11 | The model calibration has assigned detritivores to 0% feeding on the sediment bed. Polychaetes such as *Nereis virens* are included in this group, and feeding preferences of polychaetes suggest some sediment feeding. Nielsen et al. (1995) determined that *Nereis virens* can be considered benthic feeders, stating that "They use their powerful jaws as predators or scavengers…or obtain nourishment by swallowing the uppermost sediment layer with its content of detritus and microbenthic algae." Therefore, please revise the model calibration and Table 3-11 to assign detritivores a non-zero sediment bed feeding preference. | Benthic invertebrate diet parameters will be reconsidered based on the dispute resolution over exposure depth (see Response to Comment 571). | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Benthos, Calibration |
| 584 | Appendix P | Specific | Section 3.3.3, page 47, Table 3-11 | Catfish are carnivorous bottom feeders that eat benthic invertebrates such as worms that reside in the sediment. Assigning the deposit feeder proportion of prey to 0% with no possible calibration of non-zero consumption in this category is not supported by the data. For example, Tófoli et al. (2013) indicates that catfish are known to feed on deposit-feeding oligochaete worms. Please recalibrate the model using a non-zero proportion of deposit feeders consumed by catfish and revise the table once this recalibration has been performed. | Catfish consumption of deposit feeders will be set to a non-zero value as requested. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Feeding Preference |
| 585 | Appendix P | Specific | Section 3.3.3, page 50, Table 3-11 | Refer to **Comment No. 568**. The dietary preferences of bass are not consistent with the abundance data presented in Figure 3-3. Based on Table 3-11, bass are set to consume an equal amount of planktivores (filter-feeding fish) as small forage fish (each representing 40% of their overall diet), whereas the data presented in Figure 3-3 indicate that there are 11 times as many small forage fish as planktivores (88% vs. 8%, respectively). As stated in the dietary preference rationale text in Table 3-11, "the actual dietary portions are likely based on the availability and abundance of these types of small fish in the LPRSA." Please revise the bass feeding preferences to properly reflect prey availability. | See Response to Comment 384. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Feeding Preference |
| 586 | Appendix P | Specific | Section 3.3.4, page 51, first sentence | Data passed to the bioaccumulation model were averaged over the calibration period of 2011-2013, as stated in Appendix C: Use of CFT Model Data For Calibration. Because empirical tissue samples were collected from the LPRSA in 2009/2010, the calibration period should not include 2011 sediment exposure concentrations, which include post-Hurricane Irene data. Please recalibrate the model using a 2009/2010 CFT calibration or remove post-Hurricane Irene exposures from the data passed to the bioaccumulation model. | Calibration inputs from the CFT model will be changed to the 2009 to 2010 period as requested. In doing so, the CPG is considering shifting the CFT short-term calibration period to commence in WY2009 (currently WY2011 to WY2013), so that 2009 and 2010 would be modeled using the mapping of the "2010" dataset as the initial condition. Although this dataset spans 2005 through 2013 and consequently includes post-Irene samples that were collected to fill data gaps, it is likely the most realistic representation of contemporary surface sediment conditions available and a better representation than the long-term simulation's prediction of this IC. This topic will be raised in the ongoing interactions with Region 2 on CFT model topics. | The proposed revision to the short term calibration period should be discussed. This period will be alright to use as long as it does not include an artificial transient in exposure concentrations resulting from re-initializing the bed.<br><br>Also note that any seasonal transients in concentration should be considered for the steady state calibration given the seasons when fish were collected.<br><br>Categories: Bioaccumulation Model, Model Consistency<br>Sub-Categories: Bioaccumulation Linkage, Time Variable |
| 587 | Appendix P | Specific | Section 3.3.4, page 52, second paragraph and Table 3-12 | Please explain why whole-body samples for bullhead (n=6), shad (n=3), small forage fish (n=4), mummichog (n=18), perch (n=22), and benthic invertebrates (n=19) were omitted from the weight-of-evidence calibration approach. These species must be included within the calibration approach. | See Response to Comment 409. | See EPA response to comment 409: "Even though some of these organisms have lower sample sizes, this line of evidence should not be removed from the model calibration assessment." |

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| | | | | | | Categories: Bioaccumulation Model<br>Sub-Categories: Calibration Targets |
| 588 | Appendix P | Specific | Section 3.4.1.2, page 59 | The CPG's calibration technique is not typical and does not prove that a linked-model setup would be properly calibrated. The calibration applied uses a steady-state model that averages 3 years of contaminant exposure results. Without further testing, the CPG wishes to apply this model to a time-series model to evaluate rates of change within biota.<br><br>A calibration to a steady-state model could be a reasonable first step in model calibration. However, a required next step would be to display time-series results from the linked CFT and bioaccumulation models and to compare these results to all empirical data at the time these data were collected. Time-series results would also be required to assess whether fish tissue predictions are reasonable in terms of rates of change and variability in tissue concentrations.<br><br>Furthermore, as stated in Comment No. 586, EPA has significant concerns about the spatial and temporal averaging of input data and fish tissue data. For example, the CFT model data is first averaged monthly and then averaged again over the 3-year period of 2011 to 2013. This results in a comparison of model results that include post-Hurricane Irene sediment concentrations in the modeled contaminant exposures. However, all tissue data were collected pre-Hurricane Irene.<br><br>Please present time-series results as part of the model calibration report. If a steady-state component is also to be included, comparisons of sediment exposure to tissue concentration must be as temporally synoptic as possible, as stated in **Comment No. 586**. | Although the CPG agrees that time-series data would be useful in the model calibration process, insufficient data exist for this level of calibration (multiple calibration datasets for different points in time are not available). A discussion of the available data and its applicability to time-series analysis will be included in the revised report. | CPG is suggesting the application of a different time-series model after calibrating a steady-state model. EPA will need to see time-series model results to ensure that the model is set up properly and that dynamic (kinetic) processes are properly represented prior to accepting a model calibration as ready for application.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration, Time Variable |
| 589 | Appendix P | Specific | Section 3.4.1.2, page 60, last paragraph, last sentence | EPA requires that the calibrated non-steady-state model be tested prior to determining the calibration as final, among many other requirements (such as consideration of additional empirical data and the use of more complex spatial binning). Therefore, please remove the statement that "the calibration was considered final." | Text will be revised as requested. | Response accepted, pending review of the revised text.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration, Time Variable |
| 590 | Appendix P | Specific | Section 3.4.2.1, page 62, Table 3-14 | Calibration of invertebrate parameters such as AE and $K_M$ without any comparison to invertebrate data (BSAFs, site-specific data, or otherwise) means that these parameters were calibrated solely to achieve model predictions that match fish tissue concentration data. Given uncertainties in feeding preferences, this portion of the model is overly uncertain. As discussed in **Comment Nos. 575 and 595**, EPA does not consider that the benthic invertebrate model is reasonably calibrated and directs that this portion of the model be reworked or replaced (including comparisons to BSAFs or observed data) to produce reasonable BSAF predictions. | Comparison of predicted benthic invertebrate tissue concentrations to lab data is inappropriate as part of the calibration evaluation. The inferred BSAFs for benthic invertebrates are consistent with the literature. Region 2's comparisons to New York/New Jersey Harbor data cannot be evaluated beyond what the CPG has already provided in comments on the FFS because these data still have not been provided by Region 2. | EPA maintains that modeled BSAFs are too high, see EPA direction in comment 575.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Benthos, Calibration |

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| 591 | Appendix P | Specific | Section 3.4.2.1, page 63, Table 3-14 | An empirical factor that is chemical-specific and site-specific, the "particulate ventilation constant," cannot be appropriately added to the carp model. This parameter has no numerical basis in data or literature studies and allows the CPG to simply set the carp tissue concentrations to whatever the data show, regardless of whether the exposure pathway for this species is appropriately represented, as discussed in **Comment No. 578**. Please recalibrate the model without this construct. | See Response to Comment 569. | See EPA response to Comment 569.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Particulate Ventilation |
| 592 | Appendix P | Specific | Section 3.4.2.2, page 66 | The calibrated model performance is reasonable based on the metrics chosen; however, the breadth of metrics chosen is not wide enough, certain tissue comparisons have been needlessly omitted (invertebrates and forage fish), and other tissue comparisons have been rendered meaningless by the selection of excessively wide ranges for spatial and temporal averaging. Furthermore, an investigation by HDR uncovered many deficiencies with the model linkage procedure, including the equations used for averaging and calculation of bioavailable concentrations in the water column. Because the model does not account for chemicals sorbed to DOC in the water column, it dramatically overestimates the bioavailable water column concentration. In addition, EPA does not accept a 2-cm averaging depth or the inclusion of post-Hurricane Irene water column concentrations in the model calibration for comparison to pre-Hurricane Irene tissue data.<br><br>As proof that this model calibration is not unique or well constrained, HDR produced "corrected" bioaccumulation model inputs based on the CPG's CFT model results from July through September 2009 (a better temporal match to much of the fish-tissue data) and ran these inputs through the bioaccumulation model. "Corrections" included setting the sediment averaging depth to 15 cm and correcting bioavailable water concentrations (refer to Comment Nos. 361 and 541; HDR used an aDOC of 0.08 and KOW of 6.35 to compute freely dissolved contaminant rather than assuming no partitioning to DOC). With no additional calibration, the CPG model produced nearly identical results. The original underprediction in the sediment bed, using an averaging depth of only 2 cm, was nearly completely offset by the original overprediction in the water column concentrations due to incorrect partitioning and the inclusion of post-Hurricane Irene data. Diagrams of the "corrected" model results compared to the original results are shown below. | Model inputs will be revised to 2009 and 2010. Carbon partitioning will be re-evaluated. | EPA will assess the revised calibration and spatial bin assumptions when received.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Sediment Exposure Depth, Calibration, Time Variable |

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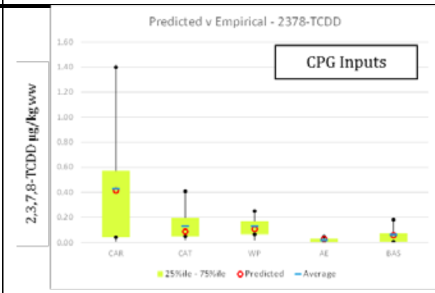
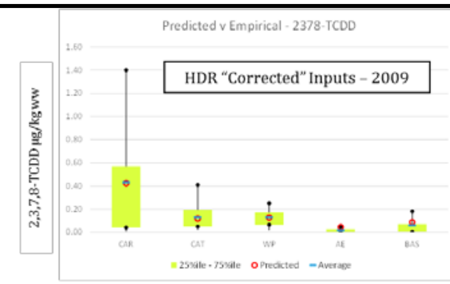
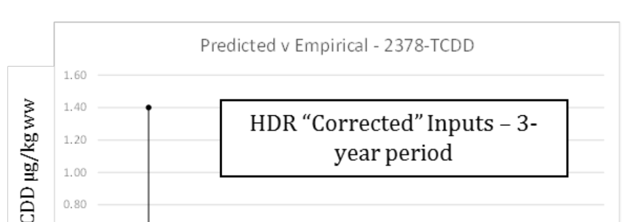
| | | | |   CPG calibration with CPG inputs; same results shown in Figure 3-18. Empirical and model-predicted whole-body data for 2,3,7,8-TCDD (excluding blue crab).   CPG model with HDR "corrected" inputs (CFT results for July through September 2009) | | |



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| 592 | Appendix P | Specific | Section 3.4.2.2, page 66 | Another test was run using the same 3-year period and spatial averaging as the CPG, but again "correcting" the sediment depth to 15 cm and the calculation for the water column bioavailable concentration. Remarkably, the calibration again looks nearly identical, as shown in the figure below. Water column concentrations in this new test were higher than the 2009 time period due to the inclusion of storm events. However, averaging water temperature over the entire time period resulted in a lower chemical uptake that offset the higher exposures. In summary, the CPG must present a model calibration in which water temperatures are dynamic and in which tissue calculations are compared with model predictions for an appropriate time period. It is clearly possible to calibrate this model with deeper sediment exposure depths.<br><br>Based on all of these considerations, the evidence presented by the CPG is not sufficient proof that this model calibration is unique or appropriate. The model requires additional calibration and testing at different spatial and temporal resolution prior to its application for remedial alternatives. | The CPG will do additional calibration work and will test the performance of alternative calibrations. The CPG will also evaluate temperature sensitivity of the model as requested. | EPA will assess the revised calibration and spatial bin assumptions when received.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Sediment Exposure Depth, Calibration, Time Variable |
| 593 | Appendix P | Specific | Section 4.1.1.1, page 73, Table 4-1 | The model significantly overpredicts tissue concentrations in small filter-feeding fish. This is likely associated with model overpredictions of water column concentrations due to the inclusion of Hurricane Irene in the calibration period, and could also be related to other parameters and feeding preferences chosen. Please recalibrate the model to correct this overprediction. | Filter-feeding fish results will be considered as part of recalibration. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration |
| 594 | Appendix P | Specific | Section 4.1.1.2, page 73, last sentence (continued on page 74) | The statement that "the small home range and foraging area of these small forage fish mean that the data are not necessarily representative of concentrations that would be present in small forage fish throughout the LPRSA" supports EPA's assertion that spatially averaging all tissue concentrations through the entire LPRSA is a flawed calibration procedure and results in the omission of data and RM trends that would be instructive for model calibration. Please recalibrate the model to include small forage fish and average data in appropriate spatial areas (perhaps within 2-RM bins). Refer to **Comment No. 577**for more specific direction regarding home range determination and the model-to-data comparison. | The CPG will expand our evaluation of how well the model predicts small forage fish concentrations on smaller spatial scales as part of the additional calibration work that will be performed in order to be responsive to Region 2's comments. See Responses to Comments 408 and 574. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration, Spatial Variability |

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| 595 | Appendix P | Specific | Section 4.1.1.4, page 75, second paragraph, third sentence | As noted in this section, the benthic invertebrate model is dramatically overpredicting 2,3,7,8-TCDD concentrations based on comparisons to laboratory data. This is just one of several lines of evidence that the benthic invertebrate model for 2,3,7,8-TCDD is not properly working and should potentially be replaced by a BSAF approach. Other lines of evidence that the benthic invertebrate model is significantly overpredicting 2,3,7,8-TCDD concentrations include:<br>• Comparison to BSAF data from HydroQual (2007) and the Canadian Council of Ministers of the Environment (2001). As noted in Comment No. 575, model predictions result in a BSAF of 2.0 to 2.5 for 2,3,7,8-TCDD. The Canadian Council of Ministers of the Environment (2001) indicates that for 2,3,7,8-TCDD, observed BSAFs range from 0.03 to 0.85 in fresh water. Multiple literature sources were examined (e.g., Loonen et al. 1997; Muir et al. 1992; Servos 1996), and none support BSAFs greater than 2.0 for benthic invertebrates; rather, most suggest that this number should be lower than 1.0.<br>• Predictions of 2,3,7,8-TCDD concentrations in deposit feeders are higher than all fish tissue predictions, including carp.<br>• As shown in Table 4-4, model predictions of 2,3,7,8-TCDD in benthic invertebrates are too high by a factor of 23 to 33 when compared to mean concentrations from the bioaccumulation investigation, and by a factor of 4 to 10 when compared to the maximum concentrations measured. The statement that these overpredictions "could be an indication that laboratory worms had a more effective mechanism for limiting exposure or uptake or eliminating 2,3,7,8 TCDD than that assumed in the model" is an admission that available data suggest that the model assumptions are not accurate.<br>Therefore, the extremely high uptake predicted for deposit feeders in the model is refuted by the best available data. Please recalibrate the model using an approach that will result in BSAFs within guidelines found in the literature and revise the text and table accordingly. | Discussion of comparison of modeled BSAFs to observed BSAFs can be added to revised report. See Response to Comment 590. | EPA maintains that modeled BSAFs are too high, see EPA direction in comment 575.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Benthos, Calibration |
| 596 | Appendix P | Specific | Section 4.1.2.2, page 78 | Small forage fish should be included in the primary model calibration and not relegated to the uncertainty analysis. The 2,3,7,8-TCDD data in Table 4-6 indicate that fish tissue data vary by RM bin, so model-to-data comparisons should be evaluated on that basis. Please recalibrate the model to include small forage fish and revise the text and table accordingly. | See Response to Comment 594. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration Targets |
| 597 | Appendix P | Specific | Section 4.1.2.2, page 79, Table 4-6 | As discussed in **Comment No. 596**, Table 4-6 shows that there is a relationship between small forage fish tissue data and RM, as well as a relationship between observed sediment concentrations and RM. Strong relationships with RM have also been observed in tissue data for bass, carp, and American eel, as shown in **Attachment 4, Figures 6a through 6e** (refer to **Comment No. 574**). For all of these organisms, a calibrated model must | See Responses to Comments 408, 574, and 594. | EPA will assess the revised calibration and spatial bin assumptions when received.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration, Spatial Variability |

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| | | | | be produced that is a function of RM; a single model calibration for the 8ntire LPRSA is not acceptable. Please recalibrate the model taking these relationships into account and revise the text and table accordingly. | | |
| 598 | Appendix P | Specific | Section 4.1.2.3, page 80, first paragraph, fifth sentence | It is understandable that increasing the depth of exposure, without changing anything else within the calibration, results in higher fish tissue concentrations and negatively affects the existing calibration. However, as discussed in **Comment No. 592**, correcting the averaging period and the water column bioavailable fraction would result in little additional required calibration and reasonable tissue concentrations, while assuming a deeper sediment averaging depth. This type of "one parameter at a time" sensitivity analysis does not provide any additional information in the calibration report; Section 4.1.2.3 should be removed. | As part of work to address comments regarding the recalibration of the model, the sensitivity analysis section will also be revised. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Sediment Exposure Depth, Calibration |
| 599 | Appendix P | Specific | Section 4.1.2.4, page 82, second sentence | Removing the near-bottom particulate layer from the calibrated model and noting that the results differ does not provide useful information. There is no evidence that the model cannot be calibrated without the addition of the near-bottom particulate layer and no observed data for this layer, making it another unconstrained portion of the model with regards to chemical concentrations sorbed to this layer. Furthermore, it is unclear why the near-bottom particulate layer is used as a surrogate for the "fluff layer," which is explicitly mechanistically modeled within the CFT model (refer to **Comment No. 570**). This type of "one parameter at a time" sensitivity analysis does not provide any additional information in the calibration report; Section 4.1.2.4 should be removed, or an alternative calibration without the fluff layer should be presented. | As part of work to address comments regarding the recalibration of the model, the sensitivity analysis section will also be revised. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration, Fluff Layer |
| 600 | Appendix P | Specific | Section 4.1.2.5, page 84, Figure 4-2 | Figure 4-2 is not an adequate replacement for comparisons of fish tissue predictions to fish tissue observations within RM bins. Please produce such figures so that the model calibration can be evaluated for each stretch of the river. As mentioned in **Comment No. 574**, there are strong trends in fish tissue and sediment concentrations by RM. The favorable comparisons shown in Figure 4-2 could be a result of the model significantly overpredicting observed data in some reaches and significantly underpredicting in other reaches, resulting in what looks like, but in reality is not, an acceptable model calibration when everything is averaged together. | Model predictions will be evaluated within smaller reaches. See Responses to Comments 408 and 574. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration, Spatial Variability |
| 601 | Appendix P | Specific | Section 4.1.4, page 88, Table 4-11 | As is the case with all tissue-to-data comparisons used for the model calibration, Table 4-11 ignores the strong correlation between RM and fish tissue concentrations (refer to **Comment No. 574**) and instead presents ratios between the entire habitat-area exposure concentrations and the average fish tissue concentrations. Please redo this analysis by comparing fish tissue concentrations with spatially relevant sediment concentrations, which also have strong trends by RM, and revise the table accordingly after this analysis is complete. | Model predictions will be evaluated within smaller reaches. See Responses to Comments 408 and 574. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration, Spatial Variability |

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| 602 | Appendix P | Specific | Section 4.1.4, page 88, second paragraph, and page 89, first sentence | The “high ratio of tissue-to-sediment concentrations for carp” could be a function of the carp feeding on sediments, including those deeper than 2 cm, rather than near-bottom particulates. This would indicate a closer relationship between carp tissue and sediment concentrations than is predicted by the model. Furthermore, the statement that “if the selective feeding hypothesis were true, the model would tend to underestimate the effectiveness of a targeted remedy designed to remove higher concentrations of 2,3,7,8-TCDD in surface sediment” acknowledges that the empirical “particulate ventilation constant” for carp could have an effect on the selected remedy and has no mechanistic basis (refer to **Comment Nos. 569 and 578**). Please replace this empirical factor with other calibration methods. | See Response to Comment 569. | EPA disagrees that contaminant uptake via respiration during feeding has been proven to be an important exposure pathway for carp. See EPA response to 569 and 578.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Particulate Ventilation |
| 603 | Appendix P | Specific | Section 4.1.5.1, page 90, first bullet, second sentence | The statement that “The average tissue concentration from these six samples can be considered as representative of the bass modeling area, because the samples were collected using an unbiased sampling design” ignores the strong trend by RM in bass samples as shown in **Attachment 4, Figure 6e**. Therefore, the average concentration cannot be considered representative of the entire modeling area; rather, the modeling area must be spatially separated. Refer to **Comment No. 577** for specific recommendations. | Region 2 has clearly expressed the desire to see the model calibrated on a smaller spatial scale. The CPG will work on that though the CPG will be attentive to the uncertainties that are generated by trying to calibrate at a smaller scale. See Responses to Comments 408 and 574. | EPA will assess the revised calibration and spatial bin assumptions when received.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration, Spatial Variability |
| 604 | Appendix P | Specific | Section 4.1.5.2, page 91, third sentence | The dietary preferences of smaller eels are quite different from those of larger eels, as documented in Appendix H. The fact that “the chemistry data for the smaller eel were not substantially different from those for the larger eel” may be due to the smaller eels, which feed directly from the sediment, consuming more contaminated sediments for a shorter time (based on Table 2 in Appendix H). Because their dietary compositions are notably different, small eels would be expected to respond differently than larger eels to remedial alternatives that clean up the sediment faster than the water column, or vice versa. Therefore, please recalibrate the model to explicitly include small eels and revise the text accordingly. | Small eel will be included in model. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration Targets |
| 605 | Appendix P | Specific | Section 4.2.2, page 96 | The water temperature sensitivity analysis is incomplete as it does not include temperatures over 17.5 degrees Celsius (°C). The CPG model has a threshold of 17.5°C for increased growth dilution effects, and water temperatures in the LPRSA can exceed 17.5°C during summer months. Please reproduce the sensitivity analysis using temperatures above this threshold. Furthermore, consider replacing the hard temperature threshold for growth dilution effects with a curve, as this formulation is not realistic. Water temperature is a sensitive value in these models as it affects bioenergetics. Averaging water temperature over 3 years is not an appropriate way to calibrate a bioaccumulation model. Time-series model results must be evaluated as part of this model calibration; refer to **Comment No. 588.** | Temperature sensitivity analysis will be revised. Insufficient data exist for time series calibration (see Response to Comment 588). Existing growth equation is based on Arnot & Gobas two-tiered temperature-dependent equations. Model sensitivity to the Arnot & Gobas temperature dependency equations will be evaluated and the use of the equation will be reconsidered. | EPA is not convinced that a steady-state model calibrated with a single temperature is an appropriate way to evaluate calibration when the applied model will not be steady state and will have a temperature threshold.<br><br>CPG must present a year or two of time-series results from the time-variable model they are proposing to use for model projections. These results can then be compared with steady-state results and observed data (even if all available data occur in the same season.).<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration, Growth Rates, Time Variable |

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| 606 | Appendix P | Specific | Section 4.2.3, page 99, first sentence | The statement that "tissue concentration predictions are sensitive to $K_M$, particularly for species for which the $K_M$ distribution range is large" indicates that the model is not well constrained. Based on this highly uncertain parameter, the CPG model can easily be made to fit observed fish tissue concentrations given a large variety of exposure concentrations and pathways, whether they are properly represented or not. Please explore alternative calibrations for this parameter and consider how these would affect different remedial alternatives. | Alternate calibrations involving different metabolism parameterizations will be explored. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Metabolism |
| 607 | Appendix P | Specific | Section 4.2.5, page 103, first paragraph, fourth and fifth sentences | All of the sensitivity analyses presented in this report are "marginal sensitivity analyses" that have limited value in terms of evaluating the model calibration. However, these sensitivity analyses generally suggest that many alternative model calibrations are likely possible. For example, if the food web was improperly specified or if feeding exposure depths were improperly set, there are enough uncertain and sensitive variables that the model could be made to produce a "reasonable" calibration despite its underlying flaws. Please replace the marginal sensitivity analyses in this report with a discussion of alternative calibrations. | Alternate calibrations will be explored. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration |
| 608 | Appendix P | Specific | Section 4.2.5, page 106, first bullet | The fact that changes to the input chemical concentration in sediment "had a relatively small impact on the overall model results" is due to feeding preferences that were based on a flawed analysis of the biomass of deposit feeders as compared to detritivores, as discussed in comments on Appendix E and Appendix H (refer to **Comment Nos. 614 and 615**). Therefore, this sensitivity analysis, suggesting that the model is not sensitive to changes in chemical concentrations in sediment, is itself flawed. Please revise this analysis and the associated text once the necessary changes regarding the feeding preferences are made. | Sensitivity analysis will be redone after benthic biomass calculations are revised. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Benthos, Calibration |
| 609 | Appendix P | Specific | Section 5, page 108, second paragraph | As discussed in previous comments, there are significant flaws in the bioaccumulation model calibration approach, the data averaging techniques, the parameters selected, and the addition of unproven empirical factors to the model. Please recalibrate the model using, at a minimum,<br>1. Time-series model results (**Comment No. 588)**<br>2. RM bins for fish tissue exposures and comparisons to observed data (**Comment No. 577**)<br>3. Deeper sediment exposure depths (**Comment No. 592**)<br>4. Appropriate selection of time periods when comparing tissue concentrations to empirical data (e.g., post-Irene exposure predictions should not be compared to pre-Irene tissue samples; **Comment No. 586**)<br>After these changes are made, EPA will reevaluate whether the bioaccumulation model "is a reliable tool for the evaluation of remedial scenarios." Until this determination is made, please remove this text from the report. | Responses to the various components of this comment are addressed as follows:<br>1. See Response to Comment 588.<br>2. See Responses to Comments 408 and 574.<br>3. See Response to Comments 571 and 592.<br>4. Ok; see Response to Comment 586. | EPA responses may be found in cited comments and responses.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration |

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| 610 | Appendix P, Appendix B | Specific | Appendix B, Section 2.3.4, page 10, Equations 20 and 21 | More information about the basis for the model's growth-rate allometrics is required. The Arnot and Gobas (2004) paper and the citations it references in support of its allometric estimations do not clarify whether these estimations are based on fish, invertebrate, or mixed data, nor the range of weights to which these estimations should apply. Allometric equations can be in error when they are applied at the edge of their domain or when extrapolating below measured organism weights. For benthic invertebrates, given the poor performance of the model discussed in **Comment No. 595**, the absence of site-specific organism weights, and uncertainty as to whether this allometric formulation was derived using invertebrate data, species-specific growth rates should be used in place of the Arnot and Gobas (2004) estimations. | A literature review will be done to determine the most appropriate grown-rate allometrics. These will be updated as needed. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Growth Rates |
| 611 | Appendix P, Appendix C | General | | Please modify the approach to passing data from the CFT model to the bioaccumulation model by making the following changes (refer to **Comment No. 361** and **Attachment 3**):<br>• Calculate particulate concentrations for each layer of the water column by dividing volumetric sorbed chemical concentrations by volumetric solids concentrations prior to averaging over depth. The current approach of averaging the volumetric sorbed chemical concentrations and volumetric solids concentration over depth first, then dividing the two averages, does not reproduce the depth-averaged particulate concentration.<br>• Use CFT model outputs for the "fluff layer" instead of the bottom layer of the water column for modeled species that feed upon the "fluff layer."<br>• Include partitioning to algae in both the CFT and bioaccumulation models and ensure that representation in the two models is consistent. The current approach assumes no partitioning to algae in the CFT model, but includes partitioning to algae the bioaccumulation model (refer to **Comment Nos. 371, 373, 398, 557, and 591**).<br>• Include partitioning to DOC in both the CFT and bioaccumulation models and in both the water column and the sediment. Currently, partitioning to DOC is only considered in the sediment in the CFT model (refer to **Comment Nos. 536 and 565**).<br>• Spatially and temporally average inputs to reflect the patterns in space and time observed in the exposure and tissue data (refer to **Comment Nos. 574, 577, 587, 588, 589, 596, 598, 603, and 608**).<br>• Average sediment concentrations over an appropriate depth (to be determined as a result of the currently ongoing dispute resolution). | 1. The requested change to the order of operations for calculating the vertically averaged solids-normalized contaminant concentration could over-weight particulate concentrations associated with very low solids concentrations. Presumably, the fraction of solids taken in from each layer by a filter-feeding fish is proportional to the fraction of total solids mass in that layer, and consequently, it seems appropriate to also weigh the solids-normalized concentration by the fraction of solids mass in each layer. The present approach effectively does this by taking the ratio of the vertically averaged sorbed contaminant mass to the vertically averaged solids concentrations, corresponding to the total sorbed contaminant mass normalized by the total solids mass. This topic will be raised in the ongoing model interaction with Region 2.<br>2. See Response to Comment 570.<br>3. Carbon partitioning will be reassessed.<br>4. Same as item 3.<br>5. See Responses to Comments 408 and 574.<br>6. See Response to Comments 571 and 592. | The CPG's concern under item number 1 is noted and should be discussed at ongoing model meetings.<br><br>The other model and documentation changes will need to be fully evaluated by R2 to assure they are adequate and appropriate once completed.<br><br>Categories: Model Consistency<br>Sub-Categories: Bioaccumulation Linkage |
| 612 | Appendix P, Appendix C | Specific | Appendix C, Section 1, page 1, first bullet | Although monthly averages were provided, those values were averaged into a single 3-year value without weighting based on the number of days in each month, and the time period over which those values were averaged (October 2010 through September 2013) did not match the time of the data collection (August and September 2009 and June, July, and August 2010). The sediment data and the CPG's model suggest that the exposure concentrations were higher in the period after Hurricane Irene (August 20- | See Response to Comment 586. | See Comment 586.<br><br>Categories: Bioaccumulation Model, Model Consistency<br>Sub-Categories: Bioaccumulation Linkage, Time Variable |

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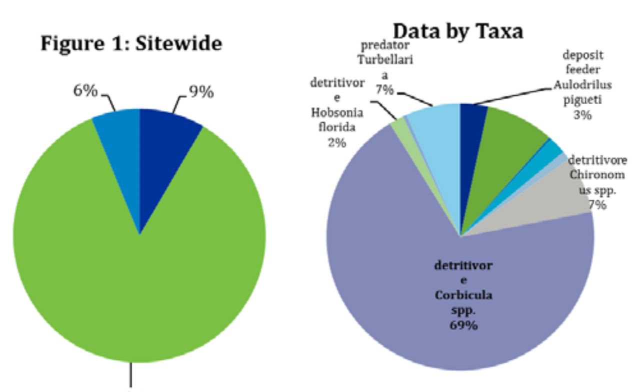
| | | | | 28, 2011) than they were when the fish data were collected. Please reevaluate the CFT model predicted concentrations used to calibrate the bioaccumulation model so that the averaging period used matches the time of the data collection and is consistent with the duration required to approach steady-state based on the model parameter values chosen. | | |
| 613 | Appendix P, Appendix C | Specific | Appendix C, Section 1, page 1, second bullet | The three spatial scales used in the calibration (study area-wide, RM 4 to Dundee Dam, and RM 7 to Dundee Dam) do not reflect the spatial gradients in the tissue and exposure concentration data. Please recalibrate the model using greater spatial resolution, which could be as fine as the eight reaches used in the 2009 Fish and Decapod Tissue Collection and 2010 Small Forage Fish Tissue Collection programs (RM 0 to RM 2, RM 2 to RM 4, RM 4 to RM 6, RM 6 to RM 8, RM 8 to RM 10, RM 10 to RM 12, RM 12 to RM 14, and RM 14 to RM 17.4). | See Responses to Comments 408 and 574. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration, Spatial Variability |
| 614 | Appendix P, Appendix C | Specific | Appendix E, Section 4.2, page 13, Figure 1 | The food web presented in the bioaccumulation model calibration report is based on the argument that the LPRSA is dominated by detritivores, or “fluff layer” consumers (refer to Comment No. 573). However, the analysis in Appendix E utilizes average weights measured in Chesapeake Bay and combines these data with abundances measured in the LPRSA, as presented in the BERA. Given site-specific differences in organism sizes based on site-specific factors and speciation, this is not an appropriate procedure to estimate biomass.<br><br>For example, Figure 1 in Appendix E suggests that detritivores make up 85% of biomass in the study area, based on abundances in the BERA. However, this figure does not illustrate that the vast majority of the detritivore biomass is composed of bivalves, specifically Corbicula. The wet weight of these clams can vary dramatically from one location to another based on age and environmental conditions.<br><br> | See Response to Comment 383. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Benthos |

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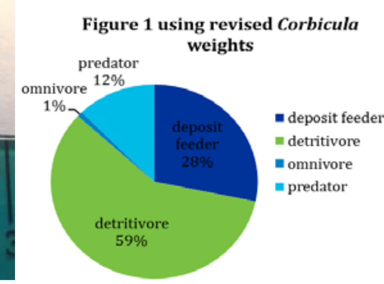
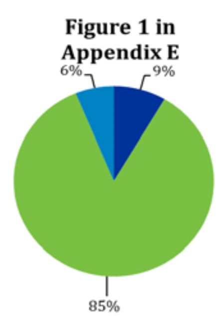
| 614 | Appendix P, Appendix C | Specific | Appendix E, Section 4.2, page 13, Figure 1 | In February of 2015, in a discussion of benthic feeding zones, the CPG presented images and specimens to EPA that suggest that typical clams in the LPRSA are tiny, with a maximum shell width of approximately 5 millimeters (mm), as shown below.<br><br>Using this picture and generic weight-to-shell-width data (Helm et al. 2004), the mean biomass of a 4-mm bivalve can be estimated as 17 milligrams (mg), which is over 3 times less biomass than estimated by the CPG using the Chesapeake Bay data. Substituting this alternate wet weight for Corbicula into Figure 1 results in the figure shown below.<br><br><br> | See Response to Comment 383. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Benthos |

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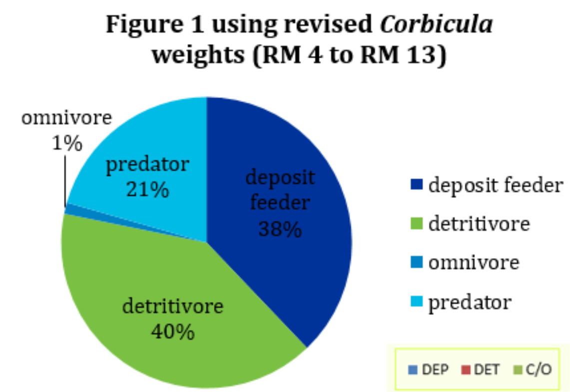
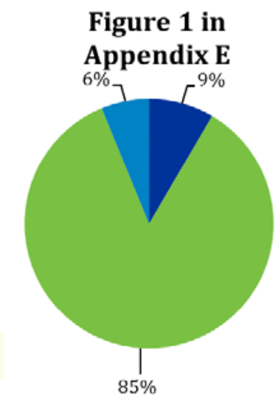
| 614 | Appendix P, Appendix C | Specific | Appendix E, Section 4.2, page 13, Figure 1 | *If the same pie chart were derived using abundance data for RM 4 to RM 13, it suggests that deposit feeders make up 38% of the benthic biomass and detritivores only 40%.*<br><br> | See Response to Comment 383. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Benthos |

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| 614 | Appendix P, Appendix C | Specific | | Changing the RM basis in this manner demonstrates how much the benthic makeup can vary spatially, and that the assumption of a study area-wide detritivore-dominated bed is not supported.<br><br>Ultimately, any process of estimating biomass based on organism weights from another site is flawed, especially as the CPG has argued that benthic organisms in the LPR exhibit unique phenotypic differences.<br><br>The variability of organism weights across even a single site requires that sample-specific data be used to measure biomass rather than using abundances and estimated mean weights. For example, in the Chesapeake Bay study alone, *Corbicula* weights ranged from 0.00005 grams to 28.26 grams. Extensive variability is also present for all other benthic invertebrates in the Chesapeake Bay study (the average difference between minimum and maximum biomasses when n > 20 is over two orders of magnitude, with the mean coefficient of variation being 151%). This variability in the (non-site-specific) data used to estimate biomasses makes the CPG's determination that the LPRSA is dominated by detritivores significantly overstated.<br><br>Finally, as illustrated in the taxa-specific pie chart above, the data used to derive Figure 1 of Appendix E suggest that the Asian clam dominates the detritivore category in terms of biomass. However, based on an examination of the literature used to convert ash-free dry weight (AFDW) to wet weight (Ricciardi and Bourget 1998), the wet weight biomass includes the shells of these clams[1], which should not be considered a viable part of prey biomass. This undoubtedly also inflates the weights of *Corbicula* relative to other portions of the food web.<br><br>Ultimately, each of these lines of evidence suggests that the CPG is significantly overstating the quantity of detritivores in the LPR and their role as prey items in the food web. The CPG must discard the flawed analysis that was used to define the proportion of benthic biomass across the LPRSA and a new analysis must be performed. As part of this new analysis, <u>shell-free</u> *Corbicula* wet weights must be estimated. If non-site-specific biomass data are used, sampling from more than one external site must be included. The CPG must also consider differences in benthic makeup by RM and derive a detritivore-to-deposit-feeder ratio for each salinity zone included in the BERA. Feeding preferences must be modified based on this new analysis (refer to **Comment Nos. 615 through 617**). Finally, if study area-specific biomass or wet weight data are unavailable, the uncertainty in the benthic invertebrate composition must be thoroughly assessed throughout model calibration and assessment of remedial alternatives. | See Response to Comment 383. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Benthos, Spatial Variability |

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| | | | | [1] AFDW from the Chesapeake Bay study is converted to wet weight using Ricciardi and Bourget (1998). However, that study states that "Wet weights of molluscs and echinoderms include their shells because they are organically connected." | | |
| 615 | Appendix P, Appendix C | Specific | Appendix H, Section 1, page 5, Table 2 | The small forage fish diet is assumed to be "65% benthic invertebrates (consumed proportionally to LPRSA biomass)." As discussed in **Comment No. 614**, the analysis performed to determine the proportion of LPRSA biomass for benthic invertebrates is fundamentally flawed as it combines Chesapeake Bay organism sizes with LPRSA abundance data. Therefore, the proportion of deposit feeders is likely much larger than the CPG's analysis suggests.<br><br>Please modify the small forage fish dietary preference for benthic invertebrates to reflect this fact. The updated dietary preferences should reflect the new analysis described in Comment No. 614. The uncertainty associated with this analysis should also be evaluated by testing alternative calibrations with a range of feeding preferences for benthic invertebrates. | Feeding preferences will be reassessed once benthic biomass calculations have been updated, and diets will be updated as needed. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Feeding Preference |

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| 616 | Appendix P, Appendix C | Specific | Appendix H, Section 1, page 6, Table 2 | The blue crab diet is assumed to be "83% benthic invertebrates (consumed proportionally to LPRSA biomass)." Similar to **Comment No. 615**, please modify the blue crab dietary preference for benthic invertebrates to include more deposit feeders, reflecting the new analysis described in **Comment No. 614**. | Feeding preferences will be reassessed once benthic biomass calculations have been updated, and diets will be updated as needed. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Feeding Preference |
| 617 | Appendix P, Appendix C | Specific | Appendix H, Section 1, page 6, Table 2 | The common carp diet is assumed to be "54% invertebrates (consumed proportional to abundance in the LPRSA)." Similar to **Comment No. 616**, please modify the carp dietary preference for benthic invertebrates to include more deposit feeders, reflecting the new analysis described in **Comment No. 614**. This should increase contaminant concentrations in carp tissue and may remove the need for an empirical factor (the "particulate ventilation constant") to fix the carp calibration. | Feeding preferences will be reassessed once benthic biomass calculations have been updated, and diets will be updated as needed. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Inputs, Feeding Preference |
| 618 | Appendix P, Appendix C | Specific | Appendix H, Section 1, page 9, Table 2 | As shown in Table 2, American eels of the smaller size class are more closely tied to the sediment in terms of dietary preferences (based on the BERA data analysis, small eels were assumed to consume 80% worms). Therefore, it is not appropriate to exclude these organisms from the model, since the two size classes of eels would be expected to respond differently to different remedial alternatives. Please revise the food web model and calibration assessment to incorporate the small size class of eels, as discussed in **Comment No. 604**. | Small eel will be included in model. | The response is accepted pending review and approval of the model revisions and revised text. EPA notes that any modeling code, inputs, results, and interpretation of results will be subject to EPA review.<br><br>Categories: Bioaccumulation Model<br>Sub-Categories: Calibration Targets |